

VOLUME 87 • NUMBER 3 • JUNE 1982

The American Historical Review

AMERICAN HISTORICAL ASSOCIATION



The Glorious Cause

The American Revolution
1763-1789

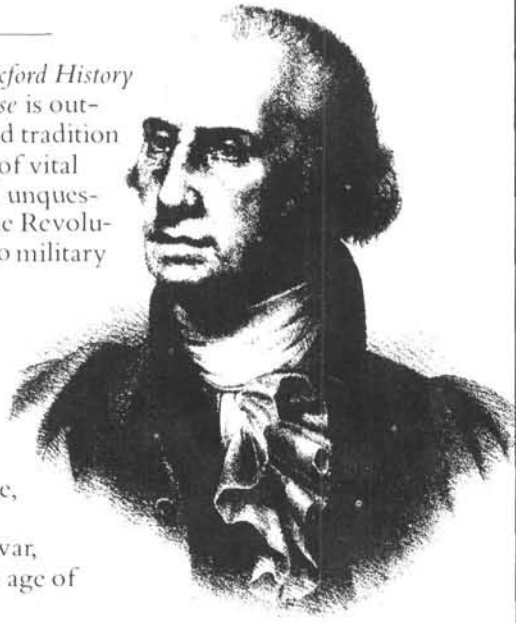
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The American Historical Review appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003 and is printed and mailed by the William Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228. The editorial offices are located in 914 Atwater, Indiana University, Bloomington, Indiana 47405.

The *AHR* is sent to members of the American Historical Association and to institutions holding subscriptions. Membership dues: For incomes over \$30,000, \$50.00 annually; \$20,000–\$29,999, \$42.00; \$15,000–\$19,999, \$35.00; \$10,000–\$14,999, \$25.00; below \$10,000 and joint memberships \$15.00; associate (non-historian) \$25.00; life \$1,000. The proportion of dues allocated to the *AHR* is \$17.00. Subscription rates effective for volume 87: Class I, *American Historical Review* only, United States, Canada, and Mexico \$43.00, foreign \$47.00. Further information on membership, subscriptions, and the ordering of back issues is contained on the two pages—1(a) and 2(a)—immediately preceding the advertisements.

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Philippine History Reconsidered: A Socioeconomic Perspective

JOHN A. LARKIN

OUT OF THE NUMEROUS REGIONAL STUDIES completed over the past fifteen years has emerged a better understanding of the socioeconomic history of the Philippine Islands. The gains have come in the area of indigenous history—that is, the history of the Islands' native population as opposed to that of the mainly foreign denizens associated with 381 years of Spanish, American, and Japanese colonialism.¹ At the heart of this fresh vision lies the notion that the most dynamic events in modern Philippine history have been the gradual attachment of the island economy to the world marketplace and the related spread of the native population onto the archipelago's vast interior frontiers.² To accommodate this enhanced knowledge, a revised temporal framework is required, a periodization that avoids the colonial bias of previous schemes and stresses the development of modern Filipino society.

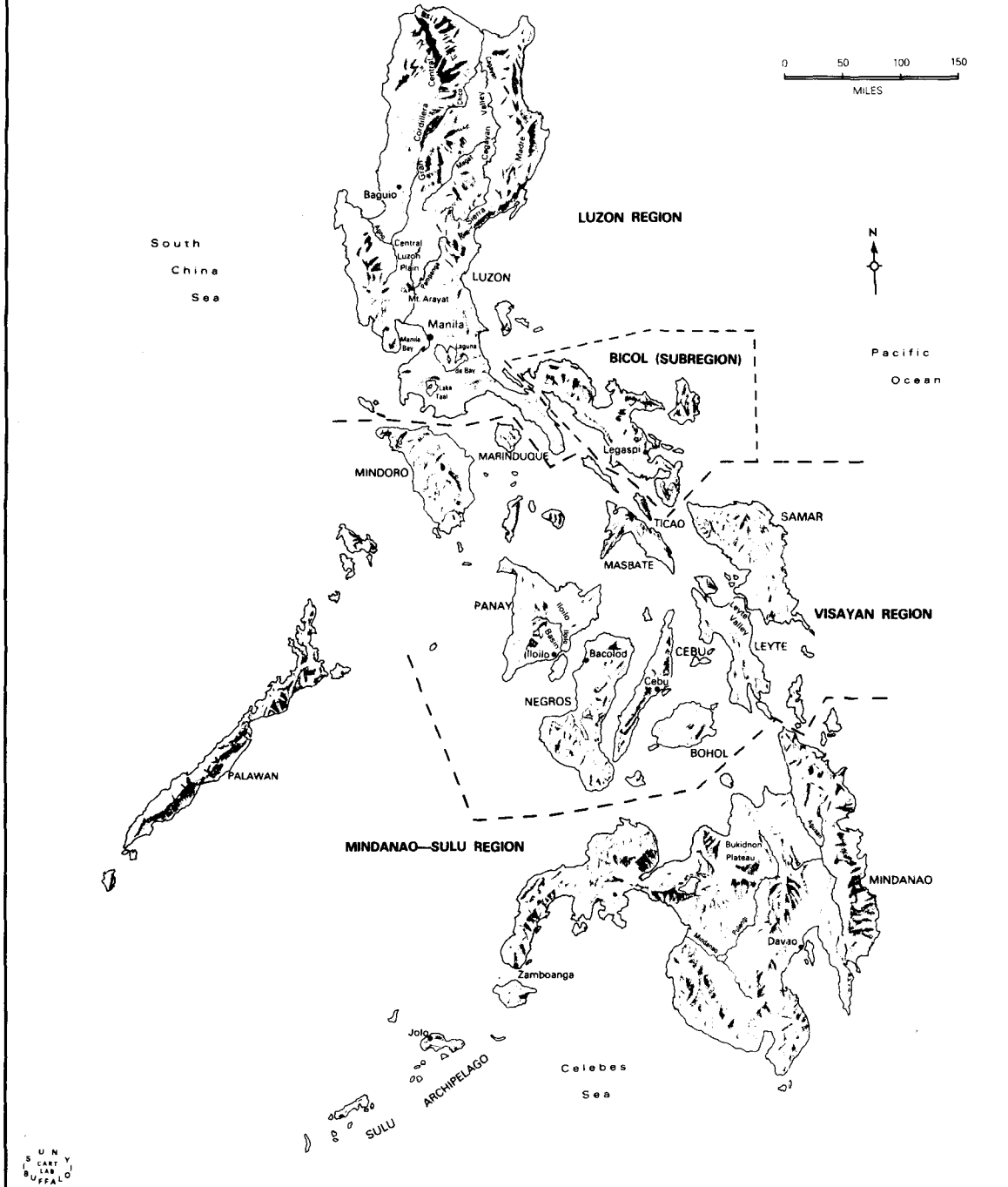
Historians indicate through their choice of periods the particular emphasis they wish to give their subject matter, and in most colonial histories they have revealed a political and administrative bias toward viewing the past, one with the colonist in the forefront. Until recently, most historians of the Philippines have employed such a schema with periodization denoting a strongly foreign outlook: Pre-Conquest Society; the Spanish Conquest and Early Colonial Era, 1565–1762; the British Invasion and Later Spanish Colonialism, 1762–1896; the Philippine Revolution, 1872–1901; American Colonialism, 1901–46; the Japanese Occupation, 1942–45;

I want to thank my colleagues Nicholas Cushner, Laurence Schneider, Benedict Kerkvliet, and Shiro Saito for their advice and support in the preparation of this essay and also Joseph Stern, Christopher Gabrielli, and Peter Smith for their assistance with the calculations. Michael Wasilenko of the Cartographic Laboratory, State University of New York, Buffalo, patiently led me through the making of the maps, and I appreciate his efforts and expertise. In forming the ideas for this article I was influenced by two pieces written by Philippine novelist Nick Joaquin, "Culture as History" and "History as Culture," which are conveniently reprinted in Visitacion de la Torre, ed., *Bilingual Contemporary Philippine Literature: Essays* (Quezon City, 1977), 1–29, 97–116.

¹ For a more extensive discussion of recent changes in the writing of Philippine history, see my "Introduction" to John A. Larkin, ed., *Perspectives on Philippine Historiography: A Symposium* (New Haven, 1979), 1–14.

² The strong impact of the world market upon Philippine society was first noted more than twenty-five years ago by Benito Legarda, Jr.; see his "Foreign Trade, Economic Change, and Entrepreneurship in the Nineteenth-Century Philippines" (Ph.D. dissertation, Harvard University, 1955), 171–249, 265–305. Only recently, however, have studies of individual agricultural regions made it apparent how pervasive and influential this impact has been.

TWENTIETH CENTURY PHILIPPINES; ISLANDS AND CITIES



Map 1

and the Independence Era, 1946—. ³ From a Manila-centric governmental vantage this sequence may seem meaningful; but within it the Filipinos have a mainly reactive role. To shift Filipinos to the foreground, to emphasize their active role, necessitates a socioeconomic perspective with its own, quite different periodization based on the growth of indigenous society.

Comprehension of the continuity of Philippine history begins with the permanent arrival of the Spaniards in the late sixteenth century. ⁴ At this time the indigenous population subsisted by hunting and gathering, farming (mainly slash and burn), and fishing. Modest external trade was confined to specific locales around the archipelago, with significant activity in the Manila, Cebu, and Sulu areas (see Map 1). The major settlements, small in size, stretched along the littorals and riverbanks that surround and penetrate the Philippine Island world. Historians must demonstrate how this simple, autonomous set of communities became modern Philippine society. At which stages was that evolution static? at which gradual? and at which rapid? What key turning point, if any, divides the premodern Philippines from the modern? And what features identify this modern society? Complicating these questions is the variety of communities that have existed in terms of their relationship to the commercial, administrative, and population centers of the archipelago. Some areas, like central portions of Luzon, gained early contact with the outside world and transformed relatively quickly; other regions, parts of the Visayas for example, continued to be isolated and quite unchanged into the twentieth century. Upland-lowland, rural-urban, commercial-agricultural, and Christian-non-Christian dichotomies that have characterized the Philippines make generalizations about modernity extremely difficult and usually incorrect. Still, certain continuities in Philippine society transcend regional variations, and some experiences Filipinos from north to south have shared.

The attachment of the archipelago to the world marketplace and the exploitation of resources on its interior frontiers are the basic forces motivating modern

³ This periodization scheme grew out of non-Spanish writings at the turn of this century, during the Philippine Revolution. See, for example, the works of two British observers: John Foreman, *The Philippine Islands* (2d edn., New York, 1899); and Frederic H. Sawyer, *The Inhabitants of the Philippines* (London, 1900). Over the next two decades, American writers made it the norm, both in school texts and in more scholarly works; see Prescott F. Jernegan, *A Short History of the Philippines* (New York, 1914); and James A. LeRoy, *The Americans in the Philippines*, 2 vols. (Boston, 1914). Filipino historians of pre-World War II also accepted it as well; see, for example, Maximo M. Kalaw, *The Development of Philippine Politics (1872-1920)* (Manila, 1926). Since World War II, this scheme appears in the major historical texts: Gregorio F. Zaide, *The Philippines since Pre-Spanish Times*, 2 vols. (Manila, 1957); Horacio de la Costa, *Readings in Philippine History* (Makati, Rizal, 1965); and Teodoro A. Agoncillo and Oscar Alfonso, *History of the Filipino People* (Quezon City, 1967). It has been used as well in the two most popular texts on Southeast Asian history: John F. Cady, *Southeast Asia: Its Historical Development* (New York, 1964); and David Joel Steinberg et al., *In Search of Southeast Asia* (New York, 1971). One attempt to change the pattern—one scheme that stresses metropolitan exploitation and the resistance to colonialism—should be noted: Renato Constantino, *A History of the Philippines: From Spanish Colonization to the Second World War* (New York, 1975).

⁴ Thanks to advances in Philippine archaeological research since the 1950s, understanding of the Philippine Paleolithic, Neolithic, and Metal Ages has improved considerably. But numerous problems of interpretation still remain to be resolved before a definitive cultural and chronological picture may safely be drawn. See Robert Fox, "The Philippines during the First Millennium B.C.," in R. B. Smith and W. Watson, eds., *Early South East Asia* (Oxford, 1979), 227-41; and Karl Hutterer, "Prehistoric Trade and the Evolution of Philippine Societies: A Reconsideration," in Karl Hutterer, ed., *Economic Exchange and Social Interaction in Southeast Asia: Perspectives from Prehistory, History, and Ethnography* (Ann Arbor, 1977), 177-96.

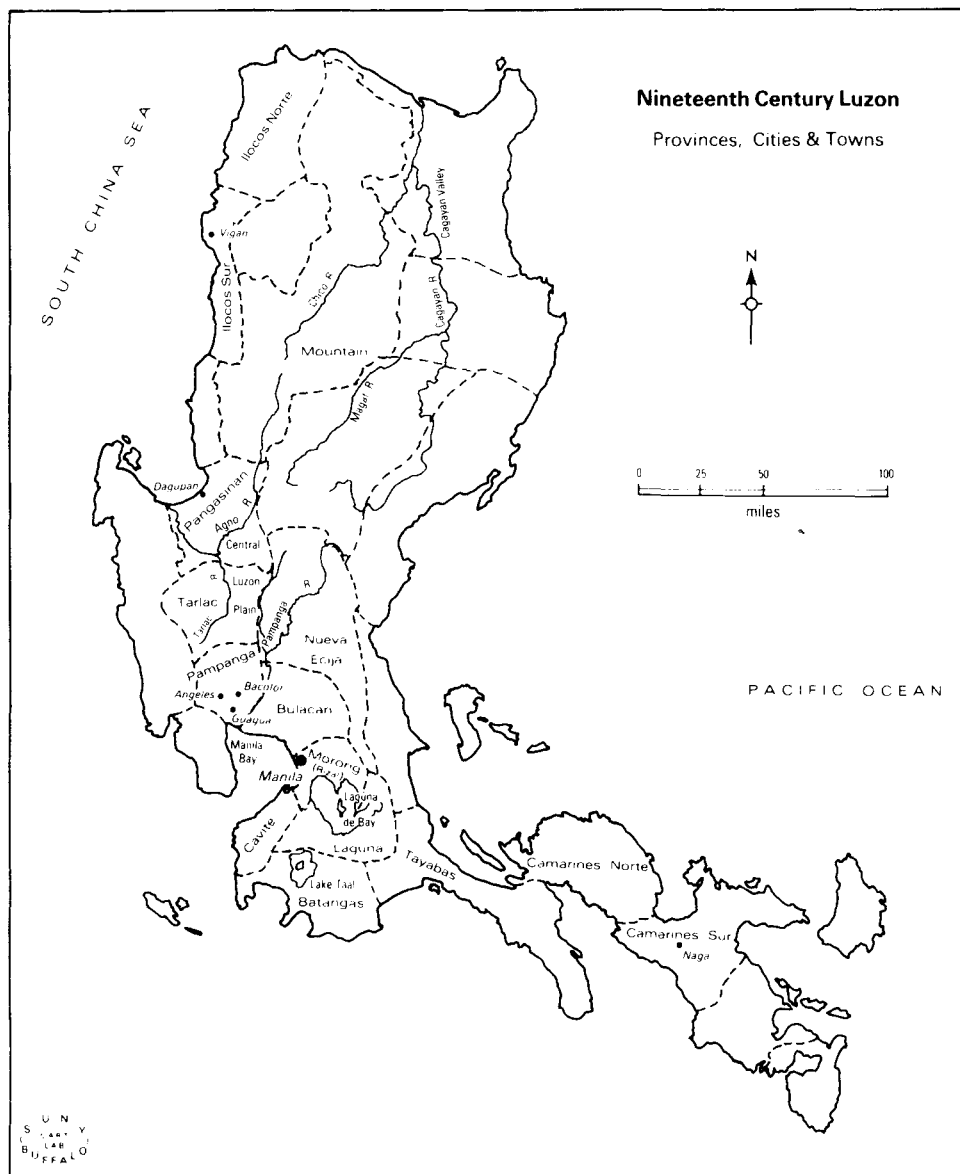
Philippine history from at least the mid-eighteenth century to the present. These two phenomena more than any others have molded Philippine society and development; they overwhelm the country's political history in significance. Admittedly, several metropolitan decisions paved the way for socioeconomic change: Spain's opening the Philippines to world commerce and the U.S. enactment of the Payne-Aldrich Tariff in 1909 both unleashed forces that shaped Philippine life. Even so, Filipino energies and ambitions released by those acts made the changes happen. Urbanization, agricultural expansion, and rising expectations have forced the political events of Philippine history during the past three centuries. Furthermore, how Filipinos relate to world commerce and how they exploit their dwindling frontier resources will in large measure determine their future.

If the role of international trade and the frontier are the central focuses, then there are four major phases in recorded Philippine history: the early period of foreign domination, when Spanish exploitation created Philippine colonial society; the mid-eighteenth to the second decade of the nineteenth century, when the Philippines moved haltingly into the world economic orbit; the following one hundred years, when the interiors of the Christian lowlands became largely settled; and the latest phase, the past half-century, when Filipinos have had to cope with changing world markets and the gradual closure of their agricultural frontier. What follows analyzes this different periodization, explaining its structure and highlighting some of the discoveries of the current generation of Philippine social historians.⁵

AFTER FOUR FALSE STARTS between 1525 and 1543, the first permanent Spanish settlement in the Philippine Islands took hold in 1565, and, gradually over the next one hundred and eighty years, the colonialists established a unique system of exploitation centered on the great port of Manila. The way in which the Spanish originally entered world commerce shaped the transformation from the pre-Hispanic native order to the society that emerged in the first two centuries of colonialism. The decision to limit commercial activity to the entrepôt trade associated with the Manila Galleon⁶ prevented development of a vast Latin

⁵ The most important of the new regional histories for this essay are Bruce Cruikshank, "A History of Samar Island, The Philippines, 1768–1898" (Ph.D. dissertation, University of Wisconsin, Madison, 1975); Daniel F. Doepfers, "Hispanic Influences on Demographic Patterns in the Central Plain of Luzon, 1565–1780," *University of Manila Journal of East Asiatic Studies*, 12 (1968): 11–96; Ronald Edgerton, "Frontier Society on the Bukidnon Plateau, 1870–1941," in Alfred McCoy and Edilberto de Jesus, eds., *Philippine Social History: Global Trade and Local Transformations* (Canberra, 1982); Brian Fegan, "Folk-Capitalism: Economic Strategies of Peasants in a Philippines Wet-Rice Village" (Ph.D. dissertation, Yale University, 1979); Bruce L. Fenner, "Colonial Cebu: An Economic-Social History, 1521–1898" (Ph.D. dissertation, Cornell University, 1976); Marshall S. McLennan, "Peasant and Hacendero in Nueva Ecija: The Socio-Economic Origins of a Philippine Commercial Rice-Growing Region" (Ph.D. dissertation, University of California, Berkeley, 1973); Norman G. Owen, "Kabikolan in the Nineteenth Century: Socio-Economic Change in the Provincial Philippines" (Ph.D. dissertation, University of Michigan, 1977); William Henry Scott, *The Discovery of the Igorots: Spanish Contacts with the Pagans of Northern Luzon* (Quezon City, 1974); and James F. Warren, "Trade, Raid, Slave: The Socio-Economic Patterns of the Sulu Zone, 1770–1898" (Ph.D. dissertation, Australian National University, 1975).

⁶ The classic work on this very wealthy, long-lived (1565–1815) triangular trade between southern China, Manila, and Acapulco is William Lytle Schurz, *The Manila Galleon* (New York, 1939).



Map 2

American type of plantation or hacienda economy and offered a large measure of administrative autonomy to native leaders. Spaniards wanted labor rather than indigenous commercial produce, and they enlisted elements of the native ruling class to organize that work force. This arrangement early on determined the bifurcated social structure that has predominated to the present.

The Spanish population concentrated in Manila, which the conquerors made their administrative and religious center, and, to a lesser extent, in such subsidiary settlements as Cebu, Vigan, and Arevalo (near modern Iloilo). In these localities the Spanish colonialists and the Chinese merchants needed food, building materials, and native labor to support their nonagrarian way of life. This requirement for



Figure 1: Bisayan Chiefs and Slaves. Drawing from a seventeenth-century manuscript account of the Islands by Francisco Ignacio Alzina, S.J., and reproduced from A. L. Kroeber, *Peoples of the Philippines* (New York, 1928), 179.

goods and services became the main and constant burden placed upon the indigenous population for the next two hundred years. For shorter terms the Spaniards made special, onerous demands. In the beginning they sought elusive sources of gold and organized native labor gangs to work the mines, and, during the protracted war against the Dutch from 1609 to 1648, they conscripted *indios* ("native Filipinos") to fight, construct warships and fortifications, and supply extra food for the campaigns. In this era, according to Spanish count, the native population decreased and only began a slow recovery in the late seventeenth century.

To support an administration that satisfied their needs, the Spaniards first installed a monopoly tax system, the *encomienda*,⁷ which proved so harsh and inefficient that they replaced it, where feasible, with a head tax (*tributo*), corvée labor (*polo*), and a levy of produce (*vandala*). Settlements along the western shores of Luzon from Ilocos to Laguna (see Map 2) and near the bishopric of Cebu experienced the strongest impact of early Spanish colonialism and were the first to be transformed under its influence. Here the Spanish clergy initially made their most energetic efforts to convert the population and concentrate it in larger communities so that *indios* could gain continuing access to their new religion. In this endeavor, religious need and secular demands for labor coincided, since more centralized settlements made organizing the population to meet corvée requirements easier. Especially pressed into service were the people from the Rio Grande de la Pampanga down to Laguna de Bay, those on the rim of Manila Bay. This process of economic Hispanization occurred far more slowly in remoter regions of the archipelago and not at all in hostile and inaccessible areas like the Gran Cordillera Central of Luzon and the Muslim country of Sulu-Mindanao.

To facilitate the exploitation of labor, Spaniards employed a native elite derived from the pre-Hispanic ruling class. These native *principales* acted as intermediaries between *indios* and their colonial overlords—collecting taxes, directing labor gangs, and leading native troop contingents. Indigenous precedent existed for the *principalia* assuming this role since labor obligation operated in pre-Hispanic society. Where once *datus* ("communal headmen") had received the services of the subservient classes, *principales* now marshaled those efforts on a grander scale for the Spaniards. In exchange, the native elite received rewards: labor and tax exemptions, titles and badges of office, and the privilege of exploiting *indio* labor for their own purposes. From the arrangement for tax collection sprang the opportunity for native officials to establish a system of debt peonage with their subordinates, and the new system replaced the old, pre-Hispanic form of debt slavery, which the Spanish outlawed before the end of the seventeenth century. Share tenancy developed, quite likely because its contractual arrangements provided the best opportunity for continuing labor exploitation.

A shortage of labor is one of the chief characteristics of premodern Philippine

⁷ On the formation of the specifically Philippine version of the *encomienda*, see Bonifacio S. Salamanca, "Background and Early Beginnings of the Encomienda in the Philippines," *Philippine Social Sciences and Humanities Review*, 26 (1961): 67–86.

society, and evidence of the Spanish need for workers abounds. Spaniards arranged tax and *corvée* exemptions for the laborers on their estates and employed share tenants as well in order to secure a constant supply of food. The government directed its attention mainly toward mustering as many workers as possible to support its economic, religious, and political endeavors. The administration created the special legal category of *vagamundos* for the landless laborers who worked in Manila and on Friar estates.⁸ Officials even sanctioned making slaves of Muslim and aboriginal peoples taken in battle. One of the prime reasons that the *encomiendas*, private and royal, persisted so long in the Philippines has to do with their relatively low administrative cost for tax collection and *corvée* labor. This form of tax farm, eliminated in the rest of the Spanish overseas empire during the sixteenth century because of its immorality and inefficiency, continued strongly in areas on the fringes of Philippine Christendom until the late seventeenth century.⁹ Assuming that Spanish census estimates are at all accurate, the population during the first century and a half of colonialism grew very slowly: in 1591, the Philippine population stood at 667,612, and, by 1735, it had only risen to 837,182, for an average annual rate of just 0.16 percent.¹⁰ This modest increase, the result of epidemic diseases, harsh *corvée* levies, Moro (Muslim malay) coastal raids, and outmigration, kept the labor burden constantly heavy upon those who endured colonial rule.

If the demand for labor continued to be high, the low premium placed on land also reflected premodern conditions. The disposition of royal land grants and the early development of Friar estates demonstrates this second reality. The Spanish crown through its early governors-general bestowed upon those who had served well during the conquest and early postconquest years land in and around Cebu and Manila. The grants were all made by 1626.¹¹ Because of the diversions of the Galleon commerce and a lack of interest in farming, few of the original grantees held on to their real estate for long, and, gradually over the course of the seventeenth century, the Friar orders and Jesuits acquired this property and organized it into quite extensive haciendas encompassing tens of thousands of hectares. Remarkably, the original grants and their consolidation into large religious holdings did not elicit any large-scale native protest. Most *indios* probably did not comprehend these transactions, based as they were on concepts of land ownership with which they were unfamiliar. Moreover, the availability at this time of large tracts of wilderness, even near Manila, prevented the estates from seeming to menace *indio* land needs. Only tenant farmers, slaves, and landless laborers who worked on the haciendas directly faced exploitation. Some native *principales* even

⁸ On all matters dealing with the Friar estates, see Nicholas P. Cushner, *Landed Estates in the Colonial Philippines* (New Haven, 1976); and Dennis Roth, *The Friar Estates in the Philippines* (Albuquerque, 1977).

⁹ Eric A. Anderson, "The Encomienda in Early Philippine Colonial History," *Asian Studies*, 14 (1976): 25–36.

¹⁰ For early census summaries, religious and civil, see U.S. Bureau of the Census, *Census of the Philippine Islands, Taken under the Direction of the Philippine Commission in the Year 1903*, 2 (Washington, 1905): 18. Figures cited later in this essay come from official Philippine census enumerations for 1918, 1948, and 1975.

¹¹ See Nicholas P. Cushner and John A. Larkin, "Royal Land Grants in the Colonial Philippines (1571–1626): Implications for the Formation of a Social Elite," *Philippine Studies*, 26 (1978): 102–11.

helped religious corporations acquire more land by selling them village communal fields and making deathbed bequests of adjoining private property.

Despite the growth of their estates, the Friar orders did not profit excessively before the late eighteenth century, since market demands and the paucity of labor limited gains considerably. Much of the hacienda property was not used at all or was merely turned to cattle grazing; only small sections were devoted to labor-intensive rice growing and sugar cultivation, mainly for local consumption. The availability of land and the scarcity of workers kept the terms of tenants' contracts relatively generous to hold *indio* labor on the estates. This condition generally persisted until the late eighteenth century, when the predominant form of tenant arrangement switched from cash leasing to share cropping, a system with more possibilities for exploitation.

Hence, the Spanish commitment to the Galleon trade, their tax system, their use of conscripted labor, and their employment of native functionaries set the pattern for rural exploitation. Native farmers and laborers were assigned to the role of support for a foreign and domestic leadership, which benefited directly or indirectly from ties to an externally oriented commerce. Once the colonial order solidified, it proved difficult for the *indios* to break away. Although the interior forests represented a vast frontier and freedom, the incentives to pioneer were lacking. Neither a land shortage nor a cash-crop market existed, and for most natives the burdens of colonialism seemed less severe than braving the treacherous, remote jungle. In the first place, the latter meant cutting ties with kin and friendship support groups. Then, pioneering required resources, including tools, draft animals, and at least one year's supply of food, and such an "establishment fund" usually came from someone in authority.¹² If a family wished to farm new land, they mainly did so in areas contiguous to existing settlements still under the sway of the colonial order. Areas farther removed from centers of power experienced less exploitation, and native society transformed more slowly; nevertheless, even there the presence of parish priests brought some degree of colonial authority. Only in the inhospitable regions was independence from the new order possible, and there that freedom was actively defended by the non-Christian peoples.

In those areas under Spanish control, shifting causes of native revolt reveal the manner in which society altered because of outside influence and internal development. Analyzing these causes helps fix the eras of transition, since rural unrest in the Philippines modifies in reaction to the impact of differing circumstances. Although such revolts may not be used to establish precise dates of change (such precision is rare in social history), they at least serve to indicate what endured and what changed across broad spans of time.¹³ The first native hostility to Spanish

¹² I have borrowed the term "establishment fund" from Brian Fegan; see "Folk-Capitalism," chap. 1.

¹³ David Sturtevant, *Popular Uprisings in the Philippines, 1840-1940* (Ithaca, N.Y., 1976); and Eric J. Hobsbawm, *Primitive Rebels: Studies in Archaic Forms of Social Movement in the 19th and 20th Centuries* (New York, 1959). I am not interested here in arguing, as Sturtevant has argued (following Hobsbawm), for a continuum of revolt from "primitive" to "modern," but, rather, in revealing the relationship between rural unrest at a given time and the differing pressures upon those who felt oppressed.

authority came at the time of the conquest, when *datus* led their followers against the conquistadors; however, this resistance was short-lived and sporadic. The autonomous, fragmented nature of pre-Hispanic society prevented large-scale joint action against more organized Spanish effort. We will probably never know whether barbarous Spanish behavior during the conquest or some vision of lost freedom motivated this initial opposition. Only the Spaniards reported these events, and they were too unacquainted with native languages to comprehend and record what their vanquished foes thought.

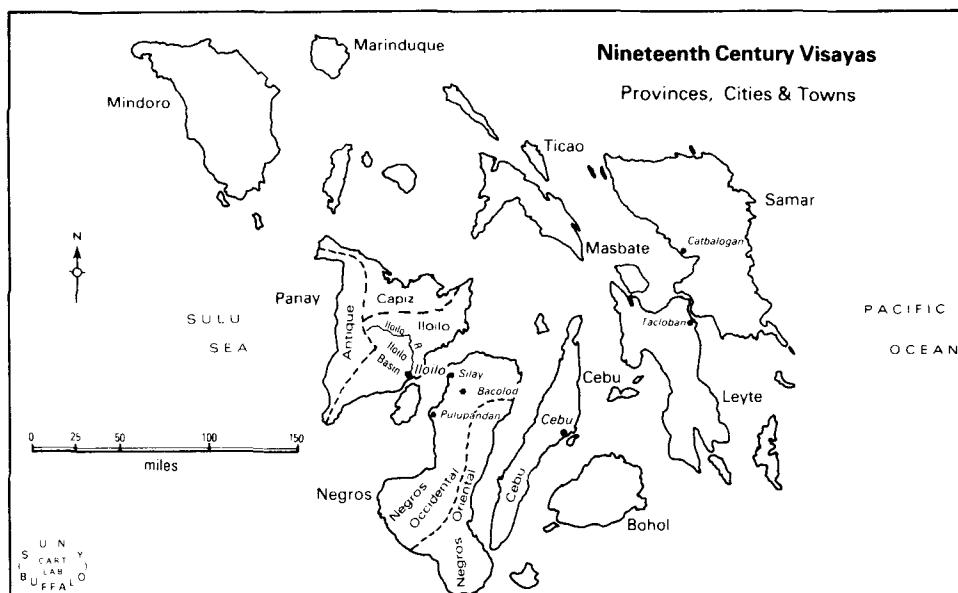
Under colonial rule, natives found ways—some covert, some open—to resist Spanish authority. The most creative and widespread method was perhaps the most subtle: they subverted the proffered Spanish culture. To describe this process John Phelan coined the term “Philippinization.”¹⁴ It refers to the transformation of Spanish norms, traits, and cultural values through the process of acculturation into a complex weave that we call Filipino culture, different in many ways from either the pre-existing culture of the ruler or the ruled. Although something of a compromise, this new amalgam stood as a rejection of the outmoded, ill-suited, medieval civilization Spanish Friars and their secular cohorts sought to impose. Natives ignored Spanish practices like infrequent bathing and asceticism, and native belief in nature spirits and pre-Hispanic rituals persisted in spite of clerical disapproval.

Overt rejection of colonial authority was a less frequent but not uncommon response during the first centuries. Christian converts to Islam, many of them originally prisoners and slaves, participated in Moro raids along the Visayan coasts beginning in the seventeenth century, and other converts served in the retinue of Muslim *datus* in Sulu and Mindanao. In Mountain Province, the expansion of the great rice terraces may well have been facilitated by the addition to the Igorot population of refugees from Spanish-controlled territory.¹⁵

Rebellion flared up as well. At least two significant patterns of revolt appeared: the first type might be called “nativistic” in that it opposed the imposition of Spanish Catholicism and sought a return to indigenous religion and culture; the second type involved protest against the labor and tax exactions of the colonial government. Pre-Hispanic religious practices have always coexisted with Catholicism in the Philippines, but on occasion priests and mediums of native sects sought to return exclusively or predominantly to the traditional ways. Sometimes these attempts at reversion led to open rebellion, as in the movement directed by Tamblot on the island of Bohol in 1621 (see Map 3). This uprising soon spread to Leyte, where a native leader named Bankaw renewed it. A similar revolt organized by one Tapar

¹⁴ John Leddy Phelan, *The Hispanization of the Philippines* (Madison, Wis., 1959), 72–89.

¹⁵ Cesar Adib Majul, *Muslims in the Philippines* (Quezon City, 1973), 121–22, 129–30, 164–70; Fox, “The Philippines during the First Millennium B.C.,” 228; Felix Keesing, *The Ethnohistory of Northern Luzon* (Stanford, 1962), 318–40; and Harold C. Conklin, *Ethnographic Atlas of Ifugao: A Study of Environment, Culture, and Society in Northern Luzon* (New Haven, 1980), 38, 98–99. In the eighteenth and nineteenth centuries, the rapid increase in population of the Balangingi Samal of Sulu, the most active slave raiders of their era, was attributed mainly to the addition to their group of large numbers of Visayan prisoners who converted to Islam; see James Warren, “Who Were the Balangingi Samal? Slave Raiding and Ethnogenesis in Nineteenth-Century Sulu,” *Journal of Asian Studies*, 37 (1978): 484–90.



Map 3

surfaced on Panay in 1663, only this time with certain Catholic syncretic elements added. The most successful of these movements, the Dagohoy Rebellion, arose in Bohol in 1744 and lasted until 1829, when followers of the original insurrectionists returned to the lowlands from the hills to which they had fled. This form of protest usually occurred in areas far from the centers of Spanish religious and secular authority and revealed the superficial nature of the acceptance of Spanish culture on the edges of the empire. As Spanish influence spread, nativistic movements became more and more tinged with foreign trappings, and, by the nineteenth century, *babailanes* (the Visayan term for “native priests”) combined their folk beliefs with Catholic ideas into a special blend of native mysticism.

A more secular form of native protest also sprang up in the first centuries of Spanish rule. These movements opposed harsh taxes and government *corvées*. In the first of these rebellions recorded, natives in Pampanga in 1584 protested the hardships that attended the forced work in Ilocos gold mines the preceding year: one missed planting season led to famine and the death of over a thousand people. Pampangans attempted to march on Manila, but they were stopped and unrest came to an end. Another insurrection took place in Samar in 1649, when the local population resisted a levy of workers for the shipyards of Cavite; led by Sumoroy, the unrest lasted for more than a year.¹⁶ Such revolts primarily challenged Friars and other officials who symbolized authority. During 1660–61, Pangasinan, Ilocos, and Cagayan became seats of protest over the harsh exactions, both labor and tax,

¹⁶ Sumoroy's revolt, which triggered smaller uprisings on islands near Samar, acquired chiliastic attributes, but it had its origins in opposition to very specific *corvée* demands that followed the conclusion of the war against the Dutch. For further details, see Eric A. Anderson, “Traditions in Conflict: Filipino Responses to Spanish Colonialism, 1565–1665” (Ph.D. dissertation, University of Sydney, 1977), chap. 6. No native revolt of this period was without both secular and religious aspects; in each case, however, one or the other aspect tended to dominate the motives, style, and activities associated with the uprising.

of the government. Rebellions continued into the eighteenth century, occurring in Pangasinan and the Cagayan Valley between 1717 and 1719 and again in Pangasinan between 1762 and 1764.¹⁷

The selective borrowing, the escape to the hills, the nativism, and the tax and corvée rebellions between the late sixteenth and the late eighteenth centuries, all illustrate early socioeconomic development in the Philippines. During these two hundred years, change took place at a variegated pace, most rapidly on Luzon, where secular protest proliferated. Visayan nativistic revolt demonstrated the limits of Spanish influence in this region, where lack of priests and Moro attacks account for the weak control. Formidable physical barriers and continuing hostility of the people of the central Cordillera and the Muslim south made fleeing Spanish authority a viable choice. Native adaptation reflected the objective realities that existed in this period: Spanish impact was superficial in many places and *indio* life changed little. Traditional occupations persisted, as did modified forms of pre-Hispanic labor exploitation under such guises as share tenancy, tax obligations, and civil and religious labor service. Spain did not intend to form, or succeed in creating, a class of native entrepreneurs who might have participated in a world commercial network. The Spaniards monopolized existing trade, setting a pattern for the mingling of political and economic power that has characterized Philippine government to the present. The internal trade that Spain did not control was left largely to the Chinese and their mestizo descendants. The *indios* had only their labor to offer.

CRUCIAL TO HASTENING THE DEVELOPMENT OF MODERN PHILIPPINE SOCIETY was the attachment of the Philippine economy to the world marketplace beginning in the late eighteenth century. This change, combined with new market conditions created by the Industrial Revolution, led to the rapid growth of the Philippine economy in the nineteenth century and fueled rapid social change. This internal social and economic transformation was, however, already in evidence by the mid-eighteenth century. At that time certain rural areas began to possess in varying degrees the elements of modern Philippine commerce and community: trade in such produce as sugar, tobacco, cord fibers, and coconut oil; regularized transport routes; regional commercial centers; and a burgeoning mestizo economic elite. The embryonic form of modern Philippine society first appeared, naturally enough, in rural areas adjacent to Manila and Cebu. Here agricultural goods filtered into the export trade long before the government officially sanctioned or even really noticed such activity. And the Tagalog Revolt of 1745 provides substantive evidence of modern social and economic attitudes in the Philippines.¹⁸

¹⁷ On these early revolts, see Sturtevant, *Popular Uprisings in the Philippines, 1840–1940*, 79–81; Phelan, *Hispanization of the Philippines*, 136–52; and Rosario Mendoza Cortes, *Pangasinan, 1572–1800* (Quezon City, 1974), 145–222.

¹⁸ Historians have tended to place the era of transition later in the century, but they base their reasoning upon Manila-centered events rather than upon what was actually happening in the countryside. Because of the influential portrayal of the Galleon trade by William Lytle Schurz, the unique episode of the British occupation from 1762 to 1764, and the decrees and policies of enlightened Bourbon officials such as Anda, Basco, and

Since prehistoric times, Southeast Asia had been a trading zone, and the Philippines were but a backwater of that trade. The Spanish Galleons only provided a slim thread tying the Islands to world markets, and those annual voyages destroyed more overseas commerce for Filipinos than they created. After Spaniards settled in Manila, pre-Hispanic native trade with China and Southeast Asia dried up in the rest of the archipelago, save in Islamic Mindanao and Sulu. But native commerce did not entirely vanish. *Encomenderos* collected native produce for sale, and, later, Spanish *alcaldes* ("provincial governors") under the privilege of the



Figure 2: Chinese Mestizos in the Mid-Nineteenth Century. Illustration from Jean Baptiste Mallat de Bassilan, *Les Philippines*, atlas vol. (Paris: 1846): plate 9, reproduced by courtesy of the New York Public Library.

indulto de commercio traded in indigenous goods. In key places at river mouths and along the shore, including Guagua, Pampanga, and Cebu, Chinese merchants established regional trade centers by the seventeenth century. They and their mestizo sons combed outlying areas in search of produce and local manufactures, sometimes for internal consumption and sometimes, via Manila, for external trade.

Viana, it has been commonplace to date the onset of modern times after the occupation, when the Manila-Acapulco run approached extinction. In actuality, international politics and the Industrial Revolution dictated the British invasion and the decline of the Galleons, while the possibilities for new directions in Philippine commerce and society sprang from the changing patterns of rural life.

Native items occasionally found their way to Mexico, and Chinese merchants took some on their homeward-bound junks. Outgoing vessels had to be outfitted and victualed, and the cordage and food derived from local sources. The arrival of the East India ship *Seahorse* at Manila in 1644 signaled the start of what became an ongoing Manila-India trade for the next one hundred and twenty years. The transpacific Galleon traffic continued as the dominant form of commerce, but it was not the only kind from the mid-seventeenth century on. French, Armenian, Portuguese, and Dutch ships, along with British Country vessels, visited Manila and made it a busy international port. Significantly, the *Seahorse* took in its return cargo samples of Philippine sugar, and later vessels supplemented their main cargoes of Mexican silver with tobacco, hides, and sugar as well as with jungle and ocean exotica and native crafts.¹⁹

The chief beneficiaries, besides Spanish administrators, of this internal and external trade were the Chinese and the Chinese mestizos who gradually fanned out into the countryside over the course of the seventeenth and eighteenth centuries. They became the middlemen serving as economic intermediaries between the *indio* and Spanish communities. The mestizos,²⁰ blending the economic skills of their Chinese fathers and the native culture of their Filipina mothers, penetrated farthest into the interior, often serving as agents for merchants of the larger settlements. Because Chinese males overwhelmingly outnumbered Chinese females in the Philippines, the mestizo population grew quite rapidly, and by the 1740s mestizos formed a discernible community with their own tax classification and their own government-recognized community leaders (*Capitanes de Mestizos Sanglayes*). The creation of a legal category for them attested to their increasing numbers and their distinctiveness from the *indio* population. They concentrated in those areas associated with internal trade: the Central Luzon Plain, Cebu, Samar, Iloilo, and Capiz. In these regions, they acquired all of the skills of middlemen and came gradually to represent the local economic elite.

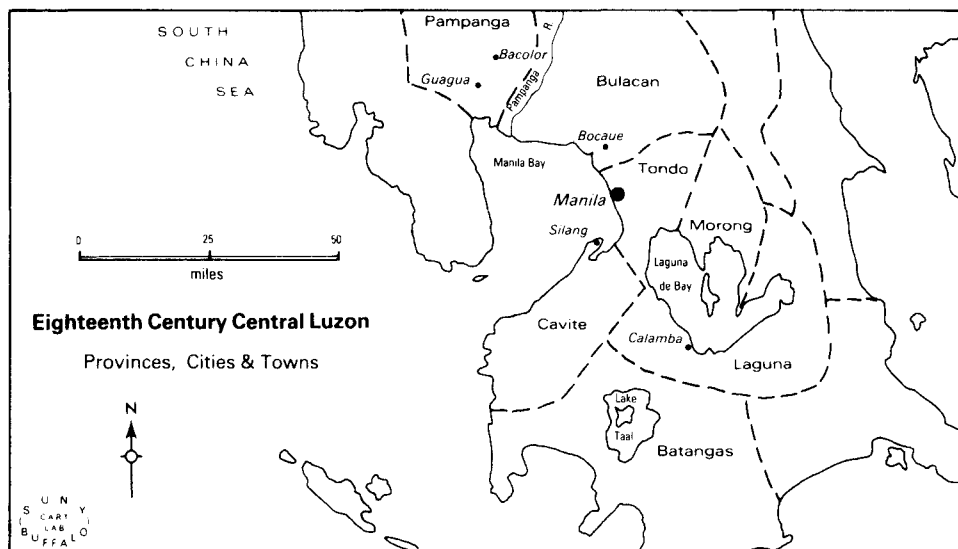
The growing presence of mestizos in rural areas suggests that mid-eighteenth-century society resembled its modern successor. It would be helpful, to flesh out this notion of a society in the early stages of modern development, to have some descriptions of contemporary native life, material culture, and behavior. Unfortunately, no studies on these subjects exist. The only things we know about the kinds of advances Filipinos made in this period are that native *principales* had enough legal training to handle their own court litigation, that a number of Philippine schools accepted *indio* students, and that a few of them went on to higher education in Spain. A good work on life in the eighteenth century is perhaps the most glaring gap in Philippine history.

Given the gradual evolution of Philippine society, it is almost impossible to set a precise date that demarcates the boundary between premodern and modern

¹⁹ Serafin D. Quiason, *English "Country Trade" with the Philippines, 1644–1765* (Quezon City, 1966), *passim*.

²⁰ I am using the unmodified term "mestizo" to refer exclusively to Chinese mestizos, and only when a modifier is added does the term refer to another group. This usage comes from Edgar Wickberg and reflects the overwhelming numerical dominance of Chinese over other mestizo groups. See Wickberg, "The Chinese Mestizo in Philippine History," *Journal of Southeast Asian History*, 5 (1964):62–100.

history; but the circumstances surrounding the Tagalog Revolt of 1745 show that, by then, modern tendencies had already become visible.²¹ The revolt extended north-south from Bulacan to Batangas and included the intervening provinces of Tondo (modern Rizal), Cavite, and Laguna: in other words, the heartland of the Tagalog-speaking people (see Map 4). The revolt lasted several months and involved many thousands of *indio* protesters as well as thousands of Spanish troops, mostly native mercenaries. Certain characteristics of the revolt and its participants speak to the incipient modernity in the Philippines. The insurrectionists came mainly from the villages surrounding the Friar estates, and they directed their wrath chiefly at estate encroachment upon their communal and private lands. The villagers also complained of having to pay for firewood and forage gathered upon upland portions of the haciendas, a fee recently added by Friar administrators. In



Map 4

their list of grievances, the principal spokesmen for the rebels noted that their lands had been seized illegally or bought at ridiculously low prices over the preceding decades. Natives had begun litigating illegal acquisition of their lands in the late seventeenth century, but, with the new round of encroachment in 1745, they felt bound to take more drastic action. Real estate was finally becoming recognized as a valuable commodity, finite in its supply, and worthy of the risk of life. Complaints, too, arose about taxes and labor obligations, an echo of former times; but the major grievance was land.

The Friars' attempt to expand the boundaries of their estates and to use nonagricultural woodlands for profit suggests their awareness of the increasing

²¹ On the Tagalog Revolt of 1745, see Roth, *The Friar Estates in the Philippines*, chap. 6; and Cushner, *Landed Estates in the Colonial Philippines*, 58–64. For the complaints of the villagers against the estates, see Lilly Library, Bloomington, Ind., Philippine MS 1745.

value of real estate. This perception of the value of land, land as a source of profit, and the actions that followed upon this perception divide premodern from modern Philippine society. *Indios* themselves had little opportunity to benefit financially from estate land, but they began to realize that others could. That realization, coupled with their own hardships, produced the protest. There are strong similarities between the Tagalog Revolt of 1745 and the Zapatista Revolt in Mexico in 1910.²²

One more complaint came from villagers in two towns, Bocaue (Bulacan) and Silang (Cavite): the power and prestige of the mestizos. In both places mestizos took over *indio* holdings or were granted special privileges. The entry of the mestizos into the agricultural sphere as landowners, a phenomenon generally associated with the following century, was already occurring, though on a small scale, by 1745. That they should then seek admission into an *indio* sphere of activity suggests that they were taking the first tentative steps toward social entry into the native elite, an integration quite complete by the end of the nineteenth century. The mestizos, although a source of friction in 1745, were also the agents of a modern economy just beginning in the archipelago.

Between 1745 and 1820, Spain sought ways to compensate for the slowly fading Galleon trade and to stem the currency flow from Mexico. Exporting Philippine commodities offered the only realistic way to make up for the lost revenues, and the archipelago presented a variety of possibilities: tobacco, sugar, indigo, and abaca along with forest and ocean wares for the specialized tastes of Europe and China. But Spaniards hoped to preserve the commerce for themselves, and in all of their initial schemes—direct trade with Spain, the Royal Philippine Company, and the Tobacco Monopoly—they sought to maintain as much command over the production and marketing of Philippine goods as possible. License for foreigners to participate in this trade was first granted clandestinely and grudgingly by local officials, and, finally, most unwillingly by a government in need of tariff revenues. Royal frigates made fourteen voyages between Cadiz and Manila from 1766 to 1783, but this attempt at stimulating direct trade ultimately folded for lack of support from Manila merchants. Similarly, the Royal Philippine Company, with its great variety of inventive schemes, failed as a result of poor planning, poor administration, and the Spanish empire's inability to sustain a mercantilistic program of this scope by the nineteenth century. The company died long before its official burial in 1834. Only the Tobacco Monopoly prospered, producing sufficient revenues to compensate for the loss of Mexican silver;²³ even so, it alone could not

²² John Womack, Jr., *Zapata and the Mexican Revolution* (New York, 1969), 41–66, 371–87. Like the Tagalog uprising, the Zapatista Revolt in Morelos, Mexico, was preceded by a period of litigious wrangling over the encroachment of large estates on village communal lands. When such litigation failed, armed conflict ensued, and, although the immediate outcome of that struggle brought some victory to the peasants, in both cases the battle over land rights continued for many more decades in the courts and in government circles. The persistence of the Tagalog and Zapatista leaders and the variety of methods they used to defend their cause identifies the modern character of their revolts.

²³ The Tobacco Monopoly, which lasted from 1782 to 1882, included government control of the growing, manufacture, and sale of tobacco and tobacco products in the Islands. For more details, see Edilberto de Jesus, *The Tobacco Monopoly in the Philippines: Bureaucratic Enterprise and Social Change, 1766–1880* (Quezon City, 1980).

make the Islands a profitable enterprise, especially for the great many Spaniards the colony needed to accommodate after the Latin American Wars of Independence. Allowing local produce free access to world trade could provide profits and revenues on a grand enough scale; it took from 1766 to 1834, however, for the government to lift the last restrictions against foreign participation in Manila commerce.

But Philippine trade did not wait for Spain to remove all economic barriers before it began to freshen. The economic currents that flowed at mid-century resumed after the hiatus of the British occupation in the 1760s,²⁴ and, by the 1780s and 1790s, Manila re-emerged as a bustling port with foreign ships and foreign resident merchants. The Tagalog provinces and Pampanga continued to forward cargoes of sugar, and the growing percentage of share contracts reveals the quickening pace of activity. Particularly on the Friar estates, change became quite noticeable. Land rose appreciably in value, and, as more fields shifted from rice to sugar, share tenancy replaced leaseholds as the predominant arrangement between landlords and tenants. Actually, on these estates a new socioeconomic structure developed, as estates began to give cash leases for large acreage to cash renters (*inquilinos*) who broke those leases down into a series of smaller sublets for share tenants. During this period Cebu City showed similar signs of revival and became again a vigorous port in the central Visayas, while its surrounding agricultural lands grew in worth. Wherever this quickening of activity occurred, mestizos asserted themselves as a dominant economic force, in part because the Chinese were temporarily excluded from the archipelago. Cebu, for example, was a city virtually run by mestizos.

Perhaps the most startling growth in Philippine commercial activity from the 1770s onward occurred in the Sulu Archipelago, when British merchants increased their demand for sea and jungle cargoes for the China trade. The resulting expansion of economic activity in Sulu stimulated a greater need for labor in that area, and Balangingi Samal and Iranun slavers raided the Visayan and southern Luzon coasts to fill that need. These Muslim pirates discouraged central Philippine shipping and inhibited coastal settlement in the Visayas and the Bicol region for a half-century.

No real expansion onto the frontier took place before 1820. Some local areas grew a little, in the immediate region around Cebu City for example, but the increase in commerce in Manila merely led to an intensification of cultivation on Friar estates. Provinces like Pampanga did not make the switch from rice to sugar as the dominant crop until the middle decades of the nineteenth century. Philippine population as a whole grew only from 837,182 in 1735 to 2,026,230 in 1818, for an average annual increase of 1.06 percent, and therefore provided no demographic need for expansion. Only Sulu, with its great increase in trade and huge influx of slaves, exhibited any real spread onto new lands. Settlement in the Philippine

²⁴ The British occupied the Philippines as part of the extension of the Seven Years' War to Asia; in actuality, however, they held only the Manila area and captured the great Manila Galleon, the *Santissima Trinidad*. See Nicholas P. Cushner, ed., *Documents Illustrating the British Conquest of Manila, 1762–1763* (London, 1971).

Islands at the beginning of the nineteenth century was scarcely wider than it was at the start of the seventeenth.

Because of the halting nature of economic growth and the small need for increased acreage, complaint over land encroachment remained minimal. The most serious protest came at the time of the British occupation of Manila, when peasant groups throughout the Islands used the opportunity supplied by the loosening Spanish grip to revolt against labor and tax exactions. Pangasinan experienced a major uprising, which lasted on and off during the entire occupation, and Diego Silang led a similar rebellion in the Ilocos region.²⁵ Revolt on a lesser scale also occurred in provinces of central and southern Luzon, the Visayas, and Zamboanga. Spanish troops suppressed all of these movements without much difficulty, save in Pangasinan and Ilocos, on the fringe of Spanish control, where a stronger tradition of protest existed. Here the government reasserted its authority in a vigorous and vicious manner, and revolution in Pangasinan did not reappear until the end of the Spanish regime. The Tobacco Monopoly also elicited a strong native reaction, especially in northern Luzon, where it functioned so pervasively. In the early nineteenth century, residents of several towns in Ilocos rose in revolt, and evasion of the monopoly's restrictions flourished in the region from Ilocos to the Cagayan Valley, particularly in the central Cordillera home of the Igorots. One minor insurrection also occurred in Ilocos Norte, where natives protested the imposition of a government monopoly on production of an alcoholic beverage, *basi*.

Until the nineteenth century, the pace of Philippine economic expansion remained limited by the demands of world commerce. Neither the lifting of Spanish rules nor the ability and willingness of indigenous producers and traders mattered so much as international demand. The case of Sulu exemplifies this point. The specific needs of the British for the China trade made Sulu flourish—indeed, flourish so well that it had to import labor on an unprecedented scale. For the next eighty years, that demand stimulated enormous growth in Sulu population, trade, and agriculture. The Industrial Revolution did not reach the Philippines before the nineteenth century—not until the Napoleonic Wars had ended. Until that time trade outside Sulu involved a wide variety of goods, many of them specialty items like mother-of-pearl, birds' nests, tapa, and black pepper. The real breakthrough in bulk trade began late in the second decade of the nineteenth century.

THE PERIOD OF MOST DRAMATIC CHANGE in all of Philippine history was the century between 1820 and 1920. The insatiable demand of the Industrial Revolution for bulk agricultural produce reached the archipelago. As world-wide mass marketing succeeded the luxury trade of preceding centuries, Philippine sugar, coffee, abaca, and tobacco replaced a panoply of exotica as the chief components of export.

²⁵ David Routledge, *Diego Silang and the Origins of Philippine Nationalism* (Quezon City, 1979). Routledge has seen—in many of the events of the Ilocos uprising of 1762–63 and in the thoughts and programs of Silang himself—evidence that the ideas and motives behind the revolt foreshadowed the nationalism of the Philippine Revolution, which occurred more than a century later.

Philippine international commerce rose from ₱4,330,965 in 1818, right after the Napoleonic Wars, to an average of ₱456,026,000 in the four years following World War I.²⁶ First Manila boomed as a port and began to make inexorable strides toward becoming a megalopolis, and, after 1855, Iloilo and Cebu, both finally opened to world traffic, turned into respectably sized ports and cities.²⁷ Foreign investments, machinery, merchants, and corporations made the Philippines part of the larger economic community during these years and permanently ended the isolation imposed by Spanish mercantilistic practice. The imported foreign goods branched out to the village level and changed the consuming habits of the native population. World commerce arrived in the Philippines during these one hundred years, and the archipelago became irrevocably tied to the rest of the globe.

Although such economic changes testify to the extraordinary shift in Philippine history, identifying this period as “the century of the frontier” is probably more appropriate: the expansion of Filipinos onto interior virgin flatlands was the most outstanding feature of transformation. Millions of hectares of new agricultural land came under cultivation, permanently altering the biosphere. Few among the indigenous population went unaffected by the activities associated with the expanding frontier, and the dominance of lowland Christian culture became firmly entrenched. The communal solidarity that had characterized pre-nineteenth-century Philippine society began to loosen before economic strains associated with the establishment of commercial agriculture, and the manner of frontier settlement indelibly marked contemporary Philippine life.

Theses and articles by regional historians document the growth of the Philippine frontier, and it is necessary here only to sketch the broad outlines of that growth.²⁸ Some regions changed more than others, while the timetable also varied depending on the confluence of events. Rising demand for sugar and abaca coincided with the suppression of Moro slave raiding and allowed settlement in the western Visayas and Bicolandia to grow, and saturated population in the older inhabited places, like the Ilocos coastal lowlands,²⁹ sent immigrants onto frontiers in the Central Luzon Plain and the Cagayan Valley. Certain rough patterns of migration shaped the process of expansion. Regions populous by the 1820s and 1830s gave up

²⁶ Legarda, “Foreign Trade, Economic Change, and Entrepreneurship,” 196; and Allen Arthur Green, “Economic Aspects of the Philippine Question” (Ph.D. dissertation, Yale University, 1939), 128.

²⁷ On the history of Philippine cities, see Daniel F. Doeppers, “Ethnicity and Class in the Structure of Philippine Cities” (Ph.D. dissertation, Syracuse University, 1972); and Robert Ronald Reed, *Hispanic Urbanism in the Philippines: A Study of the Impact of Church and State* (Manila, 1967).

²⁸ In addition to works previously mentioned, see Henry Lewis, *Ilocano Rice Farmers* (Honolulu, 1971), 21–38; Keesing, *The Ethnohistory of Northern Luzon*; John A. Larkin, *The Pampangans: Colonial Society in a Philippine Province* (Berkeley and Los Angeles, 1972); Robustiano Echaúz, *Sketches of Negros Island*, trans. Donn V. Hart (1894; new edn., Athens, Ohio, 1978); Stuart A. Schlegel, *Tiruray Subsistence: From Shifting Cultivation to Plow Agriculture* (Quezon City, 1979), 1–4; Paul Simkins and Frederick Wernstedt, *Philippine Migration: The Settlement of the Digos-Padada Valley, Davao Province* (New Haven, 1971); and Violeta B. Lopez, *The Mangyans of Mindoro: An Ethnohistory* (Quezon City, 1976). On settlement in numerous areas for which there are no full-length studies, see Frederick Wernstedt and Joseph E. Spencer, *The Philippine Island World* (Berkeley and Los Angeles, 1967), *passim*.

²⁹ Six long-settled areas—the Ilocos coast, southern Pampanga, agricultural land around Manila and Cebu City, northern Pangasinan, and southeastern Panay—seemed to reach an early saturation point in the man-land ratio in the 1820s and 1830s.



V. M. Newell

Figure 3: Photograph by M. M. Newell of Ilocanos emigrating by ox-cart. Photograph reproduced from Katherine Mayo, *The Isles of Fear: The Truth about the Philippines* (New York, 1925), opposite 52.

migrants to nearby unsettled territory, usually made valuable by the opening of a port; hence, the lands most accessible to transportation and markets filled up first. Eventually, however, more remote areas came under cultivation, though the process was slower and often involved more foreign participation.

The first real pioneering began with a northward movement of Pampangans and Tagalogs and a southward sweep of Pangasinanes and Ilocanos into interior portions of the Central Luzon Plain, including the current provinces of Tarlac and Nueva Ecija. Tagalogs also filled the Tayabas-Batangas lowlands in south-central Luzon, and Cebuanos entered the provincial areas far removed from Cebu City. Ilongo speakers of southeastern Panay overran the Iloilo Basin and central and northern Panay and, in perhaps the most astonishing move of all, settled along with other Visayan immigrants northwestern Negros Island in less than fifty years. By 1900, Negros Occidental, Panay, Capiz, Cebu, Tarlac, and much of Nueva Ecija, mostly primary jungle in 1820, were well-populated agricultural regions devoted mainly to rice and sugar. Other areas overflowed as well: Bohol and Antique, their own farmlands overcrowded by 1850, sent forth migrants who helped tame Visayan frontiers, and Bicolanos, in search of abaca lands, extended cultivation to the edges of far southern Luzon.

More remote areas, too, began to take on frontier settlement characteristics stimulated by the new economic climate emanating from the commercial cores. Many of the slash-and-burn farmers of the Bukidnon Plateau turned after 1870 to raising such commercial crops as abaca, tobacco, coffee, cacao, and gutta percha for sale to coastal merchants and inland traders (see Map 5). Samar showed signs of expansion also, but the pace there proceeded more slowly due in large measure to its inaccessibility. For non-Christian peoples the new socioeconomic climate proved disastrous. As a result of stepped-up Spanish naval and military activity, slave raiding was curbed, causing a shortage of labor and a decline in Sulu trade.

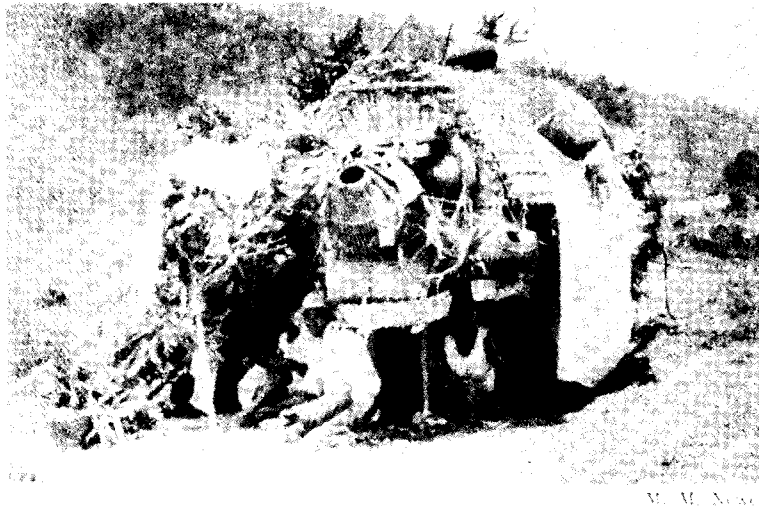


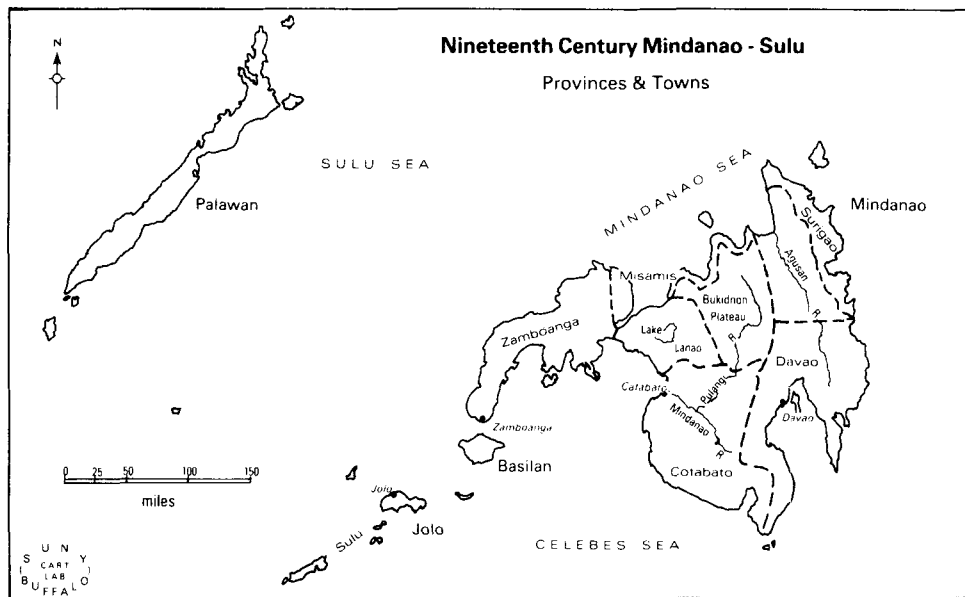
Figure 4: Photograph by M. M. Newell of emigrating Ilocanos resting during the heat of mid-day. Photograph reproduced from Mayo, *The Isles of Fear: The Truth about the Philippines*, opposite 52.

Probably, changing tastes of industrial society also made Sulu sea and jungle wares less competitive commercially. Denizens of Mountain Province experienced savage and debilitating invasions and occupations as the Spanish government sought to prevent evasions of its highly profitable Tobacco Monopoly. The history of the decimation of aboriginal populations throughout the archipelago as a result of frontier expansion merits a more thorough investigation.

The coming of the Americans stimulated expansion in some of the most isolated regions in the Philippines: cattle raising became an important business (sometimes with American participation) in Bukidnon, Ticao, Masbate, and Marinduque, while Davao turned into a major source of abaca after Japanese settlers moved into that wilderness in the early twentieth century.³⁰ Although Tacloban became an international port in 1874, Leyte Valley was not converted from jungle to rice and abaca lands until the first two decades of the new century. The upper Cagayan Valley, too, began to open up at this time, after new land became harder for Ilocano migrants to find in the Central Luzon Plain. Only particularly distant and inhospitable regions like Samar, Palawan, southwestern Negros, and interior areas of Mindanao resisted significant pioneering efforts before 1920. Mindoro, with its malarial jungles, also discouraged human habitation.

Peasants carried on the work of converting jungle into areas of permanent cultivation with the customary tools of their household—the bolo, plow, and maddox. Given the peasants' preference for communal life, solitary homesteading did not become the pioneering norm; rather, the peasants moved in groups, frequently those of townmates or kin, out into the wilderness or pushed back its edges from established settlements. From Ilocos they arrived in Nueva Ecija by

³⁰ Grant K. Goodman, *Davao: A Case Study in Japanese-American Relations* (Lawrence, Kans., 1967), 1–10, 25–32.



Map 5

wagon train, preceded by adventurous scouts and sometimes led by old community leaders. Gangs of peasant workers were offered wages or favorable tenant contracts to open land in central parts of Luzon, western Negros, and elsewhere by mestizo entrepreneurs, who often supplied them with establishment funds. Sometimes a bold patron like the Spaniard Don Agustín Montilla in Pulupandan, Negros, or the Pampangan Don Angel Pantaleon de Miranda in Barrio Culiati (the future Angeles City) personally led his tenants into the new region to begin large sugar and rice holdings and to create new towns. In the Bicol region, peasant farmers extended the frontier from old settlements to meet the demand for abaca, while slash-and-burn farmers on the Bukidnon Plateau gradually switched to sedentary farming to produce the same crop. Homesteaders taking advantage of laws promulgated by the Americans (based on similar laws from their own frontier days) became individual proprietors in remote parts of Nueva Ecija and the upper Cagayan Valley, while Japanese laborers released from road construction projects removed to Davao to become plantation hands on the new holdings of Japanese entrepreneurs. The mode of communal pioneering allowed for the sharing of tools and draught animals, necessary because most migrants were among the dispossessed, from the poorest segments of their societies; furthermore, joint action served as an important tool, physical and psychological, for coping with the inhospitable terrain.

During the century from 1820 to 1920, the newly settled areas exhibited characteristics found on such contemporary frontiers as the western United States, Australia, Russia, and Burma. The Philippines had pioneers and wagon trains, cattle ranching and rustling, cowboys and bandits, railroad building, foreign immigration, struggles with the aboriginal tribes, and alterations of the biosphere. And, as in other places, the taming of the wilderness generated enormous wealth,

although little of it benefited the pioneers themselves and the gap between rich and poor became even larger and more rigid. The economic position of the pioneers and the conditions that triggered and facilitated their activities explain in great measure why they never seriously improved their socioeconomic status. To begin with, the pioneers were among the most desperate members of society, for they came from overcrowded areas where there was no farm land or where the soil was too depleted after centuries of use. To homestead they needed an establishment fund, which could only be provided by landlords and entrepreneurs; that fund came at an exorbitant price: surrendering any possibility of a homesteader's becoming an independent farmer. Those who supplied the establishment funds found various ways to prevent the development of an independent yeomanry, which was their intent. They allowed the pioneers to work the land only as tenants and laborers in exchange for funds, or they lent cash, tools, and draught animals at rates of interest that forced the peasants to become tenants. The lack of alternate credit sources left peasants helpless to alter their condition. Sometimes, however, peasants possessed the necessary establishment funds, perhaps obtained from kin, in which case the more powerful landlords and entrepreneurs utilized land laws and brute force to coerce the independents off the land or into tenant bondage. The introduction of American homestead laws late in the frontier century did not assuage these conditions; indeed, the implementation of a cadastral survey may have only made matters worse, since peasants did not understand the intricacies of registering their land titles. In addition, peasants never controlled the marketing of their own crops, for this activity remained in the hands of native landlords and entrepreneurs and foreign businessmen; hence, the move of settlers onto the frontier only perpetuated their dependency. The desperate who pioneered early found land for a generation or two, but, when their frontier filled up, their descendants had to move to new empty spaces. Frontier social structure bore a striking similarity to society in old, established areas. The major change was that in the new territory the rich got richer.³¹

Those who prospered greatly from the exploitation of new land came from many groups in society: Chinese mestizos; Chinese; Spaniards and their mestizo descendants; the European community, mainly British; Americans; and native *principales*. For all except the last, the usual point of entry into the agricultural cycle was through collecting and shipping produce, although for some supplying credit offered the chief means. Native *principales* and, increasingly, mestizos turned to farm credit and the direct management of agricultural lands; indeed, these two groups amalgamated through intermarriage and participation in a common culture. Some Spaniards and Spanish mestizos entered the sugar industry as large landowners, perhaps because they could not compete very well in the middleman and export trade with other Europeans and the Chinese, who returned to the Philippines in 1850; however, some of the most successful

³¹ Karl J. Pelzer, *Pioneer Settlement in the Asiatic Tropics* (New York, 1948), chap. 5; and Peter Krinks, "Old Wine in a New Bottle: Land Settlement and Agrarian Problems in the Philippines," *Journal of Southeast Asian Studies*, 5 (1974): 1-17.

exporters, bankers, and agricultural processors came from Spanish families, foreign- and Philippine-born.³²

The commercial elite spread throughout the archipelago from the grandest towns to isolated plantations in the hinterland, their place of habitation tending to coincide with the special nature of their particular investments. In general, those with the widest diversity of business ventures lived in the largest communities, while those devoted to one enterprise, farming, resided in the smaller towns. The elite covered all gradations of the spectrum from extremely wealthy city dwellers to rural farmers hardly better off than their tenants. Collectively, they constituted not more than 10 percent of the population, although they possessed the lion's share of the wealth and, increasingly, all of the political power.

They operated on three levels: urban, provincial, and village. The urban elite included a mixture of Westerners and wealthy mestizos with residences in Manila, Iloilo, and Cebu City. Of the three, Manila was the clearest choice of the most powerful commercial interests in the Islands. All of the foreign firms maintained head offices there, as did such powerful clans as the Roxas-Ayala family, the Elizaldes, the Buencaminos, and the Tuazons. These family enterprises combined in various proportions shipping, agricultural processing, commercial agriculture, and the professions, and collectively they represented the most risk-oriented capitalists in the Islands—hence the diversity of their interests. Other smaller operators also lived in Manila, but the leading families and firms made the city unique. Manila provided a conducive atmosphere for their businesses because it contained all of the major credit institutions: the foreign houses, banks, and *obras pias* (those religious corporations that had a tradition of lending money dating back to Galleon days).

Manila remained the chief place of business not only because it was the center for credit and the main collection point for exports and imports but because it was the hub of information as well. Transoceanic vessels and, later, cables served as the lifelines of news about world prices and demand for Philippine commodities, while the railroad and telegraph lines brought in crop information speedily from outlying districts in Luzon. Philippine institutions of higher learning all operated in the Manila area, providing the elite with the opportunity to extend themselves into the strategic professions of law and commerce. Manila continued as the seat of governmental and judicial affairs, and easy access to official circles was essential to building large, diverse commercial empires. Local agents, factota, and country cousins ran the provincial agricultural holdings of these corporations.

Cebu and Iloilo acted as bases for a regional commercial and professional elite, usually not so diversified or large in scale as the leading ones in Manila. Mestizos residing in these two ports supplied the capital and became the plantation

³² On the economic characteristics of the nineteenth-century elite, see Rizalino Oades, "The Social and Economic Background of Philippine Nationalism, 1830–1892" (Ph.D. dissertation, University of Hawaii, 1974), chap. 3.

owners of Cebu Province, the Iloilo Basin, and western Negros. Since their interests were somewhat broad and their tastes citified, they tended to cluster in port towns rather than in farming regions, and relatives and foremen actually supervised the plantations. Foreign firms like Russell and Sturgis, Ker and Company, and Smith, Bell and Company maintained regional offices in these ports, while interisland shipping provided regular communication with Manila so that commercial information remained relatively up-to-date.

Entrepreneurs on the next level resided in such major municipal centers in the agricultural provinces as Bacolor, Pampanga; Silay, Negros Occidental; Dagupan, Pangasinan; Naga, Camarines Sur; and Catbalogan, Samar. Here a native landowning elite, heavily concentrated in agriculture, directly supervised the work of their tenants and laborers. Here, too, lived the managers of properties belonging to absentee landlords, and in Tagalog towns, like Calamba and Laguna, dwelt *inquilinos*, who leased large blocks of the Friar estates. From municipalities strategically located in southern Pampanga and northern Bulacan, investors financed the settling of the Nueva Ecija and Tarlac frontiers. As a group the towns proved attractive because they served one or more functions as regional markets, produce collection points, provincial capitals, social centers, or terminals for railroads, water traffic, or telegraph lines. They usually offered a variety of professional and business services, perhaps even a newspaper in the American period; and here the landowning elite exchanged commercial and political information, arranged marriages, and sponsored primary and secondary schools for their children.

Out in the countryside, in the *barrios* ("villages") and *sitios* ("hamlets"), lived small landholders too engrossed in the daily routines of their agriculture or not affluent enough to buy a house in the larger provincial municipalities. The elite class here gradually melded into the peasantry, and to distinguish between small landlords and rich peasants proves quite difficult. In many cases these landlords borrowed money from their richer kin while lending to those beneath them; they could be socially and economically mobile in either direction, up or down. Just occasionally a well-to-do planter family resided in the rural areas, at least for part of the year—usually during the crucial planting and harvesting seasons. Negros offers an exceptionally good example of this pattern since many of the wealthy Spanish and mestizo planters passed their days in baronial splendor on their haciendas there. Among all of the provinces in the *Census of 1903*, only Negros Occidental had some of its population data listed by hacienda rather than by *barrio*.

During the century of the frontier, the class distinctions between rich and poor grew more rigid in the wake of the formation of a new elite out of a social and economic amalgamation among the various commercial communities. Once this elite established itself, social mobility became difficult. In all settled portions of the archipelago, powerful local families increasingly fed off particular territories. Some families owned whole villages and some, major portions of provinces; once in command, moreover, these families retained their grip, and some still possess it today. There developed in the Philippines a kind of culture

of control, an idea that, in a given area, all of the land and other resources as well as almost all of the labor belonged to a single *amo*, a quasi-patriarchal landlord who dispensed justice and favors in return for the complete subservience and total loyalty of his labor force. The dispersal of this autocratic elite throughout the Islands had major consequences for the Philippines in the twentieth century, when wealthy local families converted their politicoeconomic authority into modern political power and used it to compete, usually quite successfully, with the central authority of the government.³³ The contest for control waged between provincial satraps and national officials finally brought representative government to an end in 1972.

Capital accumulated because entrepreneurs exploited both natural resources and the work force in a labor-intensive economy sanctioned and encouraged by the colonial governments. The Spaniards and Americans collected taxes and allowed the exploitation to proceed. Indeed, they often contributed to the building of infrastructure by improving port facilities, constructing internal transportation networks, and aiding the settlement of new towns. Suppressing the Moros stimulated growth in the central Visayas during the mid-nineteenth century, and containment of brigandage by the Philippine Constabulary in the twentieth century brought security to many rural areas previously plagued by disturbances. The Friars, when they converted aboriginal peoples, made a special contribution to agricultural growth; for not only did the new converts give up their claims to traditional lands more readily, but they also took up work on commercial holdings. The Americans tended to isolate aboriginal peoples to "protect" them from exploitation; in the process, however, they excluded forest dwellers from lands the aborigines formerly claimed as their own, and often such lands then fell into the hands of entrepreneurs. In addition, both colonial governments made land available at giveaway prices. Insofar as it was possible, they reduced the risks of taming the frontier.

The rich obtained their money initially through lending, transporting, and processing; and, upon becoming landowners, with the presence of consistently good markets, abundant resources, and sympathetic governments, they prospered. Between 1818 and 1918, the Islands' population rose from 2,026,230 to 10,314,300, an average annual growth rate of 1.63 percent, which guaranteed a steadily increasing supply of cheap labor. Monopoly of credit, control of information and higher education, an intricate web of strategic marriages, and a strong network of ritual kin helped the rich retain power and wealth and deny them to others. The Philippines possessed a capitalist economy well before capitalist America established its hegemony.

Given the enormous growth in capitalism during the frontier century, the Philippines' failure to develop a diversified, industrialized economy by the late nineteenth century may seem surprising. But the Philippines remained a colony, and no country under colonial domination accomplished the transition to

³³ The regional nature of the Philippine ruling class is discussed in Carl Landé, *Leaders, Factions, and Parties: The Structure of Philippine Politics* (New Haven, 1965), 4–120.

advanced industrialization. The Spanish had neither the ability nor the desire to guide such a transformation; the government remained satisfied with the status quo as long as revenues covered the salaries of the burgeoning horde of bureaucrats that plagued the Islands. They showed only a modest interest in building infrastructure. And, although the Americans eagerly supported public works, they inhibited economic development through tax schemes designed to protect U.S. commerce.³⁴ As a result of colonial policy and practice from both sides, then, the Philippines continued in its singular role as exporter of raw materials and importer of finished goods.

More than colonial rule in itself inhibited industrial growth. Entrepreneurs had little incentive for or interest in changing the nature of the economy. Foreign investors, mainly British and American, who did much to stimulate the economy in the nineteenth century, took their profits and invested them at home in the conversion of their own economies, while native entrepreneurs whose fortunes remained in the Philippines, with rare exception, showed little desire to change their pattern of investment in land. Not only was commercial agriculture a safe and familiar line of endeavor, but landowning also brought with it social prestige and political power along with easy profits. Farming also proved a good hedge against the vagaries of the world market, for landowners could expand and contract acreage according to demand and make the peasant labor force absorb the temporary losses. Within the possibilities of their situation, native entrepreneurs stayed in those economic activities that offered the broadest range of satisfaction and security.

Two additional conditions precluded diversified industrial development. First, the Philippines did not produce exclusive crops that brought steadily rising profits; rather, sugar, abaca, coffee, and tobacco had to vie with the same products from other countries, and cutthroat competition kept the lid on prices. During the century of the frontier, exports almost always exceeded imports, but, with so much capital fleeing the Islands, the margin of profits never proved large enough to launch a major technological shift in the economy. Second, the timing of events did not favor a major industrial transformation. Good market conditions only lasted roughly from 1820 to 1880; after that point, periodic depressions inhibited growth. A bad world depression after 1893 combined with unfavorable American tariff policies caused a precipitous decline in exports, and the tumult of the revolutionary era caused a further slackening of economic activity.³⁵ Recovery did not begin until around 1909, and then the war in Europe disrupted commerce intermittently until the boom of 1920–21. By the time the

³⁴ Shirley Jenkins, *American Economic Policy towards the Philippines* (Stanford, 1954), 30–41; and William J. Pomeroy, *American Neo-Colonialism: Its Emergence in the Philippines and Asia* (New York, 1970), 190–218.

³⁵ The economic downturn of the late nineteenth and early twentieth centuries was accompanied by a slowdown in population growth caused by pandemics and war-related deaths. Although the population grew at an annual rate of 1.63 percent between 1818 and 1918, from 1876 to 1903 that rate was only 1.22 percent. On the causes of this decline and long-term mortality statistics, see Peter C. Smith, "Crisis Mortality in the Nineteenth-Century Philippines: Data from Parish Records," *Journal of Asian Studies*, 38 (1978): 68–75; and Mercedes B. Concepción and Peter C. Smith, *The Demographic Situation in the Philippines: An Assessment in 1977*, Papers of the East-West Population Institute, no. 44 (Honolulu, 1977), 6–8, 14–16.

Philippine economy had matured to the point where change might have become possible, the period of unfailing good markets had come to a close. The only different investments made from then on were for expensive sugar centrals; but money, much of it foreign, spent on them only came in response to world competition, and centrals merely perpetuated the commitment to the old agricultural economy. When the frontier century came to a close, the Philippines had erected its modern social and economic structures, and from that time on has had to live with the consequences.

Quite predictably, a period of such tremendous upheaval witnessed serious social unrest following upon the dislocations and excesses associated with taming a frontier. Society underwent severe strain as social lines became more tightly drawn and as people found themselves living in unfamiliar and inhospitable terrain. The gap between haves and have-nots, especially the ostentation of the newly rich, reminded the lower classes in very vivid fashion that their own condition had not appreciably improved, may even have worsened. A century of struggle with the dangers of the frontier had produced, at most, social stasis. That crime, unrest, and protest became rampant should come as no surprise.

The causes of antiestablishment behavior are as complex as the whole frontier experience and as varied as the different regions of the archipelago. In general, there are three categories of protest against the existing sociopolitical order: banditry, folk religious movements with millenarian and utopian overtones, and the complex of movements associated with the Philippine Revolution (1872–1902). These categories are not mutually exclusive, and some of the same social rebels appeared in all three; however, the movements in each of the categories followed different courses of action and proclaimed divergent goals.

Brigandage grew especially widespread in Luzon from Nueva Ecija south to Laguna and involved holdups, pillage of isolated communities, murder, kidnapping, assault, and cattle rustling. The frontier, with its absence of controlling governmental and social mechanisms, provided a perfect environment for desperadoes, and their offenses became so serious and numerous that the colonial regime created the Guardia Civil in 1868 mainly to deal with the epidemic.³⁶ Outlaws often achieved local notoriety; they carried out their raids in densely populated agricultural districts, but they usually found hideouts in such inaccessible places as the forests around Mount Arayat, the Pampanga swamps, and the foothills of the Eastern Cordillera. It is hard to relate such banditti to any specific social cause; rather, they seem to be the fallout of every frontier from Kansas to the Hanthawaddy District of Burma.³⁷ They came from among the displaced of rural Luzon and could either be the victims of social injustice or of their own overweening greed. Until the end of the first decade of the twentieth century, they made the central area of Luzon a place of some danger.

³⁶ Theodore Grossman, "The Guardia Civil and Its Influence on Philippine Society," *Archiviana* (December 1972): 2–7; Soledad Masangkay Borromeo, "El Cadiz Filipino: Colonial Cavite, 1571–1896" (Ph.D. dissertation, University of California, Berkeley, 1973), 161.

³⁷ Eric J. Hobsbawm, *Bandits* (New York, 1972).

Folk religious movements challenged the social order in a more conscious manner, and various sects took as their objective the replacement of existing colonial society with something different. Others have described the range of such movements—their practices, culture, and aims—and have pointed out that they occurred throughout the Islands, especially on the frontiers.³⁸ Cults such as the Cofradia of Apolinario de la Cruz in the Southern Tagalog area and Guardia de Honor in Pangasinan and Tarlac donned strong Catholic trappings, while in Negros and Samar the outbursts of religious fervor and social protest bore heavily nativistic overtones centered on priests and visionaries called *babailanes* and *pulajanes*. Similar sects appeared on other frontiers around the world and reflected weakened control of more traditional orthodoxies. Various movements of the “burnt over district” in western New York and Hoa Hao Buddhism in South Vietnam provide comparable examples from other wildernesses. In the Philippines during the decade from 1899 to 1909, these movements reached a frenzy of activity. A severe economic depression, a devastating infestation of the cattle disease rinderpest, and the turmoil of the revolution left much of the countryside poor, hungry, and in need of religious solace, just when the Catholic Church found itself in a weakened state due to the exodus of its Spanish clergy. Folk religion flourished quite naturally under such duress, and only government repression and better economic conditions in the succeeding decade stemmed the tide of such fervor.

The most complex event in Philippine history was the revolution. It encompassed all of the varied political aspirations of the Filipino people and has supplied national heroes and patriotic ideals for future generations. It enlisted within its ranks a wide number of social revolutionaries who had hitherto operated independently. Felipe Salvador, “Apong Ipe” (Grandfather or Uncle Ipe) of central Luzon, especially Pampanga, Bulacan, and Nueva Ecija, epitomizes this eclecticism, for he served as the spiritual leader for his folk sect Santa Iglesia and as a colonel in Aguinaldo’s army, while the Philippine Constabulary chased down some of his subordinates because of their outlaw activities. The divergent aims of religious zealots and bandits, of Masons and urban proles, of rural landowners and peasants, became focused upon the single goal of nationhood, with little agreement on what shape that state should take. The gratuitous intervention of the United States, which aborted the revolution’s triumphant conclusion, only postponed the time when resolution of those diverse visions would have to take place.³⁹

³⁸ Sturtevant, *Popular Uprisings in the Philippines, 1840–1940*, 82–138; Reynaldo Clemenña Iletto, *Pasyon and Revolution: Popular Movements in the Philippines, 1840–1910* (Quezon City, 1979), chaps. 2, 5; Echaú, *Sketches of Negros Island*, chap. 15; and Bruce Cruikshank, *Pilgrimage and Rebellion on Samar, 1884–1886*, Wisconsin Papers on Southeast Asia, no. 3 (Madison, Wis., 1979).

³⁹ For a discussion of the complex motives of various groups participating in the Philippine Revolution, see Iletto, *Pasyon and Revolution*, chaps. 3, 4; and Milagros Camayon Guerrero, “Luzon at War: Contradictions in Philippine Society, 1898–1902” (Ph.D. dissertation, University of Michigan, 1977). On Felipe Salvador, see Larkin, *The Pampangans*, 235–39; Iletto, *Pasyon and Revolution*, chap. 6; and Guerrero, “Luzon at War,” 175–85.

Beyond the main Tagalog-speaking provinces, support for the revolutionary cause grew uncertain and divided, making general judgments about the socioeconomic implications of the upheaval difficult. Within the provinces around Manila, the disputes between Friar owners of the great estates and their tenants had much to do with the intensity of the war there. The disputes on these estates foreshadowed the menace of later socioeconomic struggles elsewhere in the Philippines. At the heart of the matter lay the problem of overcrowding, a high man-land ratio on good agricultural lands. The desire to increase profits inevitably led the Friars to squeeze the large leaseholders and, indirectly, the smaller share tenants. Large renters, such as the family of Philippine national hero José Rizal, and small tenants alike were driven off the land or had their economic position seriously undermined. Some chose to emigrate, frequently to Manila, for good virgin land in the central parts of Luzon was by now difficult to locate, and Tagalogs showed less inclination than their Ilocano or Cebuano counterparts to pioneer in distant places. Others remained on the estates in worsened straits, and the increasingly harsh conditions of servitude supplied good tinder for revolutionary fires. Revolution thus finally settled a land dispute at least a century and a half old. It also offered an early warning of further struggles over good agricultural land, which itself became overpopulated and overused in the twentieth century.

DURING THE LATEST PHASE OF THEIR MODERN SOCIOECONOMIC HISTORY, the postfrontier society from 1920 to the present, Filipinos have had to contend with erratic market conditions and with an ever-contracting frontier. In the face of a rapidly expanding population and accelerating diminution of agricultural land, Filipinos have had to resort to other sectors of the economy to soften the effects of a worsening rural standard of living. The race still continues between deriving new sources of income and growing impoverishment of the population amid the threat of mounting social unrest. If the Philippines seeks survival with good economic health into the future, its citizens will have to adjust creatively to the postfrontier, urban, international world to which they have become firmly attached. And Filipinos will have to solve the problem of making the land yield sustenance for a heavy population in ways they have never done before.

The post-World War I decade of general prosperity did not bring back normalcy in terms of old agricultural patterns. World depression struck the archipelago in the early 1930s and led to a severe curtailment of overseas trade. Following a brief period of recovery in the late 1930s, World War II destroyed the economy and left the infrastructure devastated; rehabilitation took a decade. Relatively stable political conditions since 1945 have not necessarily proved an unmixed blessing for all Philippine exports, as sugar, for example, has faced severe fluctuations in price due to intense world competition. Commodity emphasis has shifted during the last sixty years. Coconut derivatives, mainly copra and oil, have emerged ahead of sugar as the dominant export produce, and, recently, bananas and pineapples from Mindanao corporate plantations have found a vigorous outlet in Japan. Abaca, the

leading export of the nineteenth century, has faded into relative insignificance as a dollar earner.⁴⁰

Contrary markets, war, plant diseases, and changing consumer tastes have made commercial agriculture a far more speculative and expensive venture. The exhaustion of the soil in older, established areas has necessitated increased use of fertilizer, and the disruption of the water supply by indiscriminate logging has added new problems, especially for increasingly hard-pressed small farmers. An important element in the agricultural picture contributing to rising land costs is the dangerous climb in population during this period. Between 1918 and 1975, the population grew from 10,314,300 to 42,070,660, an average annual increase of 2.46 percent, in spite of the loss of life incurred during World War II; and from 1948 to 1975 the growth rate surged to an alarming 2.89 percent per year. With such a burgeoning population, a large portion of agricultural output, produced on farms of ever-diminishing size, has gone for domestic consumption. The shrinking size of the average holding has not prevented the growth of a rural proletariat and a large number of displaced farmers who have joined the swell of urban squatters around Manila, Cebu, and some other intermediate cities. More and more, the Philippines has had to depend on output from rapidly depleting forest reserves and abundant mines to garner foreign exchange.

Exhaustion of readily available frontier lands has exacerbated the demographic problem; few new territories remain that can absorb the Philippine people. Following the First World War, Nueva Ecija and the Bukidnon Plateau filled up, and, since the Second, interior portions of Mindanao, Mindoro, Palawan, Samar, and northeastern Luzon, including the Cagayan Valley, have gradually approached full settlement. The limits of land available for sedentary agriculture are being reached, and in Negros, for example, bulldozers now scrape off the tops of mountains to make a few extra fields available.

Against a background of fluctuating markets, insecure agricultural conditions, and closing frontiers, agrarian unrest has proliferated, making the past sixty years the most turbulent period in Philippine history. Both folk religious movements and secular uprisings have sprung up in the countryside, threatening the regional elite and challenging the government. Since local circumstances produced these insurrections, meaningful generalizations about their causes prove hard to establish; however, it seems that social protest steeped in native religious tradition shows up more commonly in frontier areas, and secular unrest in areas longer settled. Between 1920 and 1935, folk movements, often dubbed "Columism" by an indiscriminating government, flourished in such remote areas as Surigao in northern Mindanao, in eastern Pangasinan, and in northern Nueva Ecija. Here the sedate churches could not compete with folk religion, and peasants faced harsh natural conditions and, sometimes, land grabbing from avaricious entrepreneurs. The new sects exhibited their later formation by incorporating such national heroes

⁴⁰ No systematic study of Philippine economic history exists, and I have drawn my information from Wernstedt and Spencer, *The Philippine Island World*, chaps. 6, 7, 8; T. M. Burley, *The Philippines: An Economic and Social Geography* (London, 1973), 43–93, 248–310; and George Hicks and Geoffrey McNicoll, *Trade and Growth in the Philippines: An Open Dual Economy* (Ithaca, N.Y., 1971), chap. 4.

as José Rizal and Andres Bonifacio into their panoply of deities; in organization, size, and activities, however, they resembled groups of a century ago. In central Luzon around Mount Arayat such movements showed a marked continuity of tradition, exemplified by their common reverence for Felipe Salvador as one of their saints. Leaders of these new religions still regularly communed with Apong Ipe, although the government executed him in 1912. As a form of agrarian protest, folk religious movements faded by the mid-1930s, indicating in part the closing of the kind of frontier that nourished folk protest.⁴¹ From this point on, secular unrest and Muslim rebellion became the main forms of insurrection.

From Friar estates during the revolution, major agrarian unrest traveled north into the Central Luzon Plain by the 1920s. Pampanga, Bulacan, and Nueva Ecija experienced an increasing number of tenant protests after 1920 through such movements as Kapatiran Magsasaka, KPMP, AMT, Sakdal, and Anac Pawas.⁴² Whatever the names of the organizations, disaffected peasants supported them, often switching from one to the other without realizing they were different. The various groups coalesced, more or less, into the Socialist Party under the inspiring leadership of Pedro Abad Santos in the 1930s. Eventually, peasants from those three provinces became the backbone of the Huk movement during and following World War II. After its defeat in the early 1950s, the Huks went into eclipse and were succeeded by the New People's Army; by then, however, activities had shifted north into Tarlac, northern Nueva Ecija, and the Cagayan Valley as well as into the lower Tagalog region. Army pressures chased the rebels further away in the 1970s, into northeastern Luzon, to the Sierra Madre Range, to Samar, and to interior reaches of Mindanao.

At each stage, guerrillas operated farther and farther from Manila and more established farm regions, and, once defeated, re-emerged in new areas—never to be effective in the old ones again. This pattern of migration reveals several key features about the relationship between land and society. Social protests became most effective at a stage of economic development and change when population pressures upon agricultural land reached a point of advantage for landlords, who could lever tenants into increasingly onerous contracts. Widening social distance between landlord and tenant usually coincided with this phase of transition. Agrarian unrest both triumphed and lost. It altered the situation to the extent that landlords fled the countryside and rents dropped, but violent protest failed to change the underlying problem: the naggingly high man-land ratio. After insurrection had been quelled or had moved on, in its aftermath the imbalance between the supply of tenancies available and the number of peasants seeking land persisted. Gradually, the poor found themselves split between relatively better off tenant farmers and landless rural workers.⁴³ And, when the class interests of the poor no

⁴¹ Sturtevant, *Popular Uprisings in the Philippines, 1840–1940*, 256–66. Sturtevant noted the rise since the 1950s of folk cults, many honoring José Rizal; however, none of these sects, save one, has challenged authority. The one exception, Lapiang Malaya, fought against the Philippine Constabulary in downtown Manila in 1967.

⁴² Benedict Kerkvliet, *The Huk Rebellion* (Berkeley and Los Angeles, 1977), 26–60.

⁴³ Fegan, "Folk-Capitalism," chap. 6; Lewis, *Ilocano Rice Farmers*, *passim*; and Akira Takahashi, "The Link between Community Studies and History: Peasants in a Changing Society," in Larkin, *Perspectives on Philippine Historiography*, 47–54.

longer turned them exclusively against the landholding elite, agrarian dissent approached the point of ineffectiveness. Social action has never succeeded in Negros Occidental, for example, in spite of truly harsh and repressive conditions, because the poor have been divided against themselves into a small group of permanent plantation workers (*duma'an*) and great gangs of migrant cane cutters (*sacadas*). Conflicting interests deaden revolutionary possibilities. Those activists who seek to correct rural inequities will have to discover ways to bridge the divisions among the lower classes before they can expect to achieve effective change.

Only one other form of protest need be noted: the Muslim rebellion in Mindanao. Once again, this movement reveals complex historical antecedents, but a strong source of grievance revolves around the land-grabbing and logging of big-time political and military operators on this last frontier.

Trends indicate a bleak future for the Philippines unless an enlightened populace can find administrative and technological solutions for the threatening social and economic problems. Ecologically unsound and improvident use of the frontier has not brought well-being to the Filipino; rather, the use of the frontier has created a maldistribution of wealth, squandered valuable resources, widened the social gulf between rich and poor, and left the Islands vulnerable to painful internal dissent. Filipinos cannot be blamed for colonial mismanagement, but neither can they hide behind it in the face of impending Malthusian crisis. A physical frontier no longer exists, but a challenging mental one remains: can Filipinos ingeniously solve their demographic and nutritional problems with their remaining resources? The utilization of high-yield varieties of rice has already produced one modern breakthrough, the so-called Green Revolution, but others are needed to make the future of the Philippines brighter.

THE PERIODIZATION SCHEME SUGGESTED IN THIS ESSAY calls attention to the socioeconomic dimensions of the Philippine past. Such a framework encourages contemplation of native Filipinos living and interacting with the forces around them, rather than as negative respondents to the impositions of foreign and domestic political, economic, and administrative tyranny. The more proper way to view modern Filipinos is as innovative reactors to outside forces and creators of their own destiny within the limits of cultural and historical circumstances.

Adherents of Immanuel Wallerstein's paradigm for world systems will see in the attachment of the Philippines to the world marketplace proof of the archipelago's becoming a peripheral region tied to European, American, and Japanese core areas.⁴⁴ But his paradigm is not necessarily the most useful for understanding the development of indigenous society, for its focus is too universal and too politicoeconomic in nature to achieve that comprehension. A more socioeconomic and ecological perspective is needed to deal with the key role of Philippine

⁴⁴ For a concise discussion of the Wallerstein paradigm, see his *The Capitalist World Economy* (Cambridge, 1979), 1–36. For a useful critique of the applicability of this paradigm, see Angus McDonald, Jr., "Wallerstein's World Economy: How Seriously Should We Take It?" *Journal of Asian Studies*, 38 (1979): 535–40. Also see Judith A. Perrolle, "The Institutional Aspects of Philippine Agriculture in the 1960's," *Filipinas: A Journal of Philippine Studies*, 1 (1980): 45–73.

expansion into and reaction to the jungle wilderness. In this regard, ideas deriving from frontier theorists such as Walter Prescott Webb are better.⁴⁵ Discussions of boom theory, the role of pioneers and entrepreneurs, and land tenure and usage as well as frontier religion and culture supply a more appropriate framework in which to cast the Philippine experience. Much profit remains in exploring comparatively the Philippine past with that of other such frontier regions in Brazil, Burma, Malaysia, and Sumatra.⁴⁶

Historians of the Philippines have long concerned themselves with the formative role colonialism and neocolonialism have played in the shaping of native society and its problems. Now regional history, indigenous in focus, offers an innovative and comparative method of identifying the unique characteristics of Filipinos and their community. The periodization suggested in this essay may help the new generation of scholars recognize the archipelago-wide implications of their endeavors on the local level.

⁴⁵ Especially see Webb, *The Great Frontier* (Austin, 1964), chaps. 1–6.

⁴⁶ For comparative purposes, see Martin T. Katzman, "Social Relations of Production on the Brazilian Frontier," in David Harry Miller and Jerome O. Steffen, eds., *The Frontier: Comparative Studies* (Norman, Okla., 1977), 275–95; Stephen I. Thompson, "The Cultural Ecology of Pioneer Agriculture in Contemporary South America," *ibid.*, 296–316; Michael Adas, *The Burma Delta: Economic Development and Social Change on an Asian Rice Frontier, 1852–1941* (Madison, Wis., 1974); James C. Jackson, *Planters and Speculators: Chinese and European Agricultural Enterprise in Malaya, 1786–1921* (Kuala Lumpur, 1968); and Karl J. Pelzer, "Western Impact on East Sumatra and North Tapanuli: The Roles of the Planter and the Missionary," *Journal of Southeast Asian History*, 2 (1961): 66–71.

Republican Revisionism Revisited

ISAAC KRAMNICK

FOR OVER A HUNDRED YEARS the world of scholarship agreed that Locke was the patron saint of Anglo-American ideology in the eighteenth century and that liberalism with its stress on individuality and private rights was the dominant ideal in that enlightened and revolutionary era. For the Victorian Leslie Stephen, it was self-evident that "Locke expounded the Principles of the Revolution of 1688 and his writings became the political bible of the following century." For the more recent Harold Laski, it was equally clear that Lockean liberalism dominated English political thought in the eighteenth century. Colonial Americans, it was assumed, were also schooled on Locke and became, in fact, his most self-conscious disciples. Thus, for Carl Becker, "the lineage is direct, Jefferson copied Locke," and, for Merle Curti, the "Great Mr. Locke" was "America's philosopher." Louis Hartz has summarized this scholarly consensus. "Locke," he wrote in 1955, "dominates American political thought as no thinker anywhere dominates the political thought of a nation."¹

As it comes to all orthodoxies, revisionism has set in, and this received wisdom has been assaulted with a vengeance. Over the last twenty years a fundamental reinterpretation of Anglo-American eighteenth-century social and political thought has occurred. The liberal individualist heritage preoccupied with private rights has, to a great extent, been replaced by a republican tradition emphasizing citizenship and public participation, a tradition with roots deep in the classical and Renaissance worlds. Fundamental to this republican revisionism has been rethinking the hegemony of Locke. As Stanley N. Katz has noted,

This essay was written at the Center for Advanced Study in the Behavioral Sciences during academic year 1979–80. Many thanks go to its splendid, hospitable staff as well as to the National Endowment for the Humanities, whose grant to the center allowed me to be there. I should also thank Professor Philip Siegelman of San Francisco State, who once again was the catalyst for my work. His invitation to participate in the Western Political Science Association's Spring 1980 meetings was the prod to get some of this on paper in its original form. Professors Jack Greene and Joyce Appleby both read and criticized the completed essay. I am in their debt for their constructive comments. Final acknowledgment is due Professor J. G. A. Pocock, part of whose seminal scholarship I depart from here. That he has set the terms of discourse within which we may debate should be self-evident.

¹ Stephen, *A History of English Thought in the Eighteenth Century*, Harbinger edn., 2 (New York, 1962): 114; Laski, *The Rise of European Liberalism* (New York, 1936), and *Political Thought in England from Locke to Bentham* (New York, 1920); Becker, *The Declaration of Independence: A Study in the History of Political Ideas* (New York, 1922), 79; Curti, "The Great Mr. Locke, America's Philosopher, 1783–1861," *Huntington Library Bulletin*, no. 11 (1939): 107–51; and Hartz, *The Liberal Tradition in America* (New York, 1955), 140.

Locke et praetera nihil, it now appears, will no longer do as a motto for the study of eighteenth century Anglo-American political thought. The state of nature, doctrine of consent, and theory of natural rights were not as important before 1776 as the ideas of mixed government, separation of powers and a balanced constitution. We are only in the opening phases of a major reassessment of our constitutional heritage.²

Replacing Locke as the vital center of political discourse in the century is the country, opposition ideology of the Walpole years. In turn, these ideas are themselves read as part of a larger tradition—the civic humanist, or republican, tradition.

The revisionist school makes two distinct claims. The first de-emphasizes the role of Lockean ideas in the early eighteenth century. The second questions Locke's influence on the entire century, including the radicalism of post-Wilkes England and the ideology of the American founding. In its first claim, revisionism is on solid ground. Locke deserves the de-emphasis he has received for the early part of the century. In its second claim, however, the revisionist position is much more dubious; here it has gone too far.

THE REPUBLICAN, REVISIONIST READING has replaced Lockean liberalism with civic humanism. Part Aristotle, part Cicero, part Machiavelli, civic humanism conceives of man as a political being whose realization of self occurs only through participation in public life, through active citizenship in a republic. The virtuous man is concerned primarily with the public good, *res publica*, or commonweal, not with private or selfish ends. Seventeenth-century writers like James Harrington and Algernon Sidney adapted this tradition, especially under the influence of Machiavelli (according to J. G. A. Pocock),³ to a specifically English context. This significantly English variant of civic humanism, "neo-Machiavellianism" or "neo-Harringtonianism," became, through the writings of early eighteenth-century English Augustans like Davenant, Trenchard, Gordon, and especially Henry St. John, Viscount Bolingbroke, the ideological core of the "country" ideology that confronted Walpole and his "court" faction. Bolingbroke provided a crucial link in this intellectual chain by associating corruption with social and political themes,⁴ a critical concept in the language of eighteenth-century politics. Much richer than simple venality or fraud, the concept is enveloped by the Machiavellian image of historical change: corruption is the absence of civic virtue. Corrupt man is preoccupied with self and oblivious to the public good. Such failures of moral personality, such degeneration from the fundamental commitment to public life,

² Katz, "The Origins of American Constitutional Thought," *Perspectives in American History*, 3 (1969): 474. Also see Robert E. Shalhope, "Toward a Republican Synthesis: The Emergence of an Understanding of Republicanism in American Historiography," *William and Mary Quarterly*, 29 (1972): 49–80.

³ For J. G. A. Pocock's arguments, see his *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition* (Princeton, 1975), "Virtue and Commerce in the Eighteenth Century," *Journal of Interdisciplinary History*, 3 (1972): 119–34, *Politics, Language, and Time: Essays on Political Thought and History* (New York, 1971), and "Early Modern Capitalism—The Augustan Perception," in Eugene Kamenka and R. S. Neale, eds., *Feudalism, Capitalism, and Beyond* (Canberra, 1975).

⁴ See my *Bolingbroke and His Circle: The Politics of Nostalgia in the Age of Walpole* (Cambridge, Mass., 1968).

fuel the decline of states and can be remedied only through periodic revitalization by returning to the original and pristine commitment to civic virtue. Calls for such renewals, for *ridurre ai principii* (Machiavelli's phrase), form the response to corruption.

Bolingbroke's achievement was to appropriate this republican and Machiavellian language for the social and economic tensions developing in Augustan England over the rise of government credit, public debt, and central banking as well as for political issues, such as Walpole's control of Parliament through patronage or concern over standing armies. Themes of independence and dependence, so critical to the republican tradition (the former essential to any commitment to the public good), were deployed by Bolingbroke into a social map of independent country proprietors opposing placemen and stock jobbers and a political map of a free Parliament opposing a despotic court. In addition, Bolingbroke stamped this eighteenth-century republican-country tradition with its socially conservative and nostalgic quality, in terms of not only its anticommercialism but also its antiegalitarianism. But this court-country reading eschews class analysis, at least in terms of the conventional dichotomy of progressive bourgeoisie and reactionary aristocracy. Its categories and frames of reference are older and more complicated.

To a great extent, the innovative scholarship of J. G. A. Pocock has shaped this new way of looking at English political thought. His writings on Harrington and his magisterial *Machiavellian Moment* (1975) have made the concept of civic humanism a strikingly useful tool with which to understand the political mind of late seventeenth- and early eighteenth-century England. The more ambitious extension of civic humanism's reign, however, is questionable. In the hands of Pocock and others, like John Murrin and Lance Banning, this insightful reading of early eighteenth-century politics through Bolingbroke's dichotomy of virtuous country and corrupt court does not stop with Augustan England. It becomes the organizing paradigm for the language of political thought in England as well as America throughout the entire century.⁵ As a result, revisionism in this second claim also insists on the irrelevance of class in political discourse, which in conventional progressive or liberal scholarship has been linked to the later decades of the century via the emergence of the Industrial Revolution. Analyses of the late eighteenth century that refer to class consciousness or conflicting class ideologies or that use concepts such as aristocracy, capitalist, feudal, or bourgeois are thus dismissed by republican scholarship as simplistic and proleptic. Challenges to the "primacy" or "omnipresence" of "civic ideology," of "Aristotelian and civic humanist values," derived throughout the century not from "simple bourgeois ideology" or from visions of "economic" or "capitalist man" but from a court ideology, part commercial and part elite, that was not representative of a class in any conventional sense. There is no dialectical tension between middle and upper classes. To claim it existed is to engage in "much distortion of history." There is for Pocock only one

⁵ For Murrin, see his "The Great Inversion, or Court versus Country: A Comparison of the Revolution Settlements in England (1688–1721) and America (1776–1816)," in J. G. A. Pocock, ed., *Three British Revolutions: 1641, 1688, 1776* (Princeton, 1980), 368–455; and, for Banning, see his *The Jeffersonian Persuasion: Evolution of a Party Ideology* (Ithaca, N.Y., 1978).

proper dialectical reading, which sees everywhere “the dialectic of virtue and commerce.” All of Anglo-American political thought in the eighteenth century involves, then, “a continuation, larger and more irreconcilable of that Augustan debate.”⁶

Locke and possessive individualism in this scheme have obviously had to go.⁷ And a chorus of distinguished scholars have joined John Dunn in de-emphasizing the importance of Locke throughout eighteenth-century Anglo-American thought. “Eighteenth century English political thought,” according to Gordon Wood, “perhaps owed more to Machiavelli and Montesquieu than it did to Locke.” Indeed, Bernard Bailyn has persuasively argued that “the effective triggering convictions that lay behind the [American] Revolution were derived not from common Lockean generalities but from the specific fears and formulations of the radical publicists and opposition politicians of early eighteenth century England.”⁸

J. G. A. Pocock has been the most insistent in repudiating Locke’s influence on the entire century. He has seen the history of political thought “dominated by a fiction of Locke,” whose importance “has been wildly distorted.” He and others are engaged in what he has called “a shattering demolition of [Locke’s] myth.” Their concern is to prove that the predominant language of politics for the eighteenth century, even for its radicals, “is one of virtue, corruption and reform, which is Machiavellian, classical and Aristotelian, and in which Locke himself did not figure.” What we have come to, Pocock has insisted, is the end of “the image of a monolithically Lockean eighteenth century,” the end of “a convention of writing as if Locke dominated the thought of the eighteenth century, and imposed on it a pattern of liberal individualism.” Indeed, he concluded, to understand the debates of eighteenth-century politics does “not necessitate reference to Locke at all.”⁹

Pocock has applied this revisionist verdict about Locke to an alternative reading of America and its founding. American political culture has been haunted by myths, the most mistaken of which is the role of Locke as “the patron saint of American values.” The proper interpretation “stresses Machiavelli at the expense of Locke.” The Revolution was, Pocock wrote, “the last great act of the Renaissance . . . emerging from a line of thought which staked everything on a positive and civic concept of the individual’s virtue.” The Revolution was a Machiavellian *rinnovazionne* in a new world, “a republican commitment to the renovation of virtue.” America was born in a “dread of modernity,” according to Pocock. In its early years “the country ideology ran riot.” The debate over Hamilton’s economic policies in the 1790s “was a replay of court-country debates seventy and a hundred years earlier.” In Jefferson’s polemics, however, “the spirit of Bolingbroke stalked

⁶ Pocock, “Virtue and Commerce in the Eighteenth Century,” 130–34.

⁷ The term “possessive individualism” is, of course, C. B. Macpherson’s from *The Political Theory of Possessive Individualism: From Hobbes to Locke* (Oxford, 1962), and his reading of Locke significantly informs mine.

⁸ Dunn, “The Politics of Locke in England and America in the Eighteenth Century,” in John W. Yolton, ed., *John Locke: Problems and Perspectives* (Cambridge, 1969), 45–80; Wood, *The Creation of the American Republic* (New York, 1972), 29; and Bailyn, *The Origins of American Politics* (New York, 1972), ix–x, 56–58. Also see Bailyn’s *The Ideological Origins of the American Revolution* (Cambridge, Mass., 1976).

⁹ Pocock, *The Machiavellian Moment*, 424, “Virtue and Commerce in the Eighteenth Century,” 124, 127, 129, and *Politics, Language, and Time*, 144.

on every page.” John Murrin concurred. The Jeffersonians, he wrote, “like an English country opposition . . . idealized the past more than the future and feared significant change, especially major economic change, as corruption and degeneration.”¹⁰ Welcome from this perspective, then, is Gary Wills’s recent book on Jefferson’s Declaration of Independence. Wills, too, got rid of Locke, but, behind Jefferson, he saw not Locke but Hutcheson and the Scottish Enlightenment.¹¹

Perhaps the best summary of this more ambitious school of revisionist scholarship in making the claim for both the early and late eighteenth century can be found in Donald Winch’s recent work on Adam Smith. Winch set out to rescue Smith from those who have misread him as a theorist of individualism and liberal capitalism, from what Winch has labeled as Marxist and Whig scholarship—“Those who come to bury capitalism as well as those who come to praise it.” To so read Smith is to disregard “the remarkable body of revisionist literature” that depicts the entire eighteenth century free of Locke and free of the bourgeoisie:

Those political theorists and historians who are committed, for one present-minded reason or another, to the enterprise of constructing a genealogy of liberal or bourgeois individualism which is continuous from Locke to the nineteenth century and beyond have suffered a major casualty as a result of recent research on eighteenth century political thought and ideology. That casualty is no less a figure than Locke himself, the “founder” of liberal constitutionalism.

Winch then described “the remarkable historiographic upheaval . . . which converges on the conclusion that Locke’s *Two Treatises* [sic] was of strictly limited significance to many of the most lively as well as profound developments which took place in Anglo-American political thought during the eighteenth century.” Nor were the traditional issues of liberalism important. Political thought in the period, Winch concluded, “owed far less to Locke’s concern with questions of obligation, original contract, and natural rights than was originally thought to be the case.”¹²

It has, indeed, been a “remarkable historiographic upheaval.” Republican revisionism has sharpened our perceptions of the ideological currents operating in the eighteenth century, but its two claims must remain distinct. Revisionism has informed us of the continuity and hold of older political and cultural ideals, competing with a Lockean emphasis on natural rights and individualism, on the early eighteenth-century mind. But, in seeking to free the entire eighteenth century of Locke, of socioeconomic radicalism, and of bourgeois liberalism, this new broom has also swept away much that is truth.

There are serious difficulties in applying to Anglo-American politics after 1760 the model of court and country or the dialectic of virtue and commerce. These difficulties derive from the basic revisionist assumption of a continuous meaning throughout the century of concepts like corruption and virtue. The nostalgia,

¹⁰ Pocock, *The Machiavellian Moment*, 469, 529, 548, and “Virtue and Commerce in the Eighteenth Century,” 130–31, 134; and Murrin, “The Great Inversion, or Court versus Country,” 406.

¹¹ Wills, *Inventing America: Jefferson’s Declaration of Independence* (New York, 1978).

¹² Winch, *Adam Smith’s Politics: An Essay in Historiographic Revision* (Cambridge, 1978), 28, 36, 41, 54, 180.

hierarchicalism, and anticommercialism of the earlier part of the century cannot be that easily read into the later years of the century. A study of the writings and politics of British reformers from 1760 to 1800 illustrates the problematic nature of such a reading. Is Locke irrelevant to the reformers' radicalism? Are their ideological paradigms republicanism and civic humanism? Is theirs the politics of nostalgia untainted by bourgeois liberal or individualist ideals?

THE VERDICT OF RECENT REPUBLICAN SCHOLARSHIP is that Locke and progressive liberal ideals were, in fact, unimportant in the agitation for parliamentary reform in Britain from the 1760s through the French Revolution. Relying heavily on the work of British historians like Herbert Butterfield and Ian Christie, the revisionists emphasize the nostalgic and even reactionary quality of the reform movement.¹³ What were being sought were lost historical rights, Anglo-Saxon rights. Alternatively, the reformers were country ideologues concerned only with mixed government and an independent House of Commons. Republican scholarship denies that any social or economic motives or grievances were at work among the reformers, either democratic or bourgeois. Continuity and nostalgia are the key, not radical appeals to abstractions like the rights of man or nature. In such a configuration of ideas, Locke is seldom to be found.

For Pocock, the reform movement was simply civic humanism and country rage. "Georgian radicals in the era of the Revolutionary War and its aftermath used a language indistinguishable from that of their American peers." That same language of corruption and virtue was being used "against the ministries of George III," by the foes of Bute "and the friends of Wilkes." This was no casual flirtation with the language of civic humanism by the radicals. Pocock has noted that the country ideology of republican virtue that the Americans adopted "had originated in England and was still very much in use there. In the minds of James Burgh, John Cartwright, or Richard Price, it was as obsessive and terrifying as in any American mind." It was "the conceptual framework" behind "radical demands for parliamentary and franchise reform." In *The Machiavellian Moment*, Wyvill, Price, and Cartwright are described as using "a vocabulary of corruption and renovation little different from that of their American contemporaries." In an earlier article, Pocock placed Burgh, Wilkes, the Yorkshire movement, the Society for Constitutional Information, and, *miracula mirabilis*, John Thelwell in the tradition of country and civic humanism. They are "key points in the long continuous history of a political language and its concepts." The terminology and ideas of country ideology, Pocock has concluded, "were extensively borrowed by the radical left when one began to appear in George III's reign."¹⁴

¹³ See Butterfield, *George III, Lord North, and the People, 1779–80* (London, 1949), esp. 229–56, 337–52; and Christie, *Myth and Reality in Late Eighteenth-Century British Politics and Other Papers* (London, 1970), "Introduction" to G. S. Vetch, *The Genesis of Parliamentary Reform* (London, 1964), and Wilkes, Wyvill, and *Reform* (London, 1962).

¹⁴ Pocock, "Virtue and Commerce in the Eighteenth Century," 133, 122, *The Machiavellian Moment*, 507, 547, and *Politics, Language, and Time*, 133, 145–46. Also see Ian Hampshire-Monk, "Civic Humanism and Parliamentary Reform: The Case of the Society of the Friends of the People," *Journal of British Studies*, 18 (1978–79): 70–89.

This backward-looking reading of British reform is shared by Gordon Wood, who has also described Price, Burgh, and even Paine as members of this camp of virtue-obsessed republicans.¹⁵ Bernard Bailyn has agreed:

The leaders of the [American] Revolutionary movement were radicals—but they were eighteenth century radicals concerned, like the eighteenth century English radicals, not with the need to recast the social order nor with the problems of economic inequality and the injustice of stratified societies but with the need to purify a corrupt constitution and fight off the apparent growth of prerogative power.¹⁶

But this essay offers a very different reading of these radicals and of British reform in general between 1760 and the French Revolution. Locke was very much alive and well in their arguments.

The radicals of the later eighteenth century, both English and American, were much more likely to base their arguments on natural rights than on historical rights; they were preoccupied less with nostalgic country concerns than with very modern socioeconomic grievances. They shared a deeply felt sense that the unreformed British constitution failed to serve the interests of the talented and hard-working middle class.¹⁷ Locke was, indeed, unimportant to the earlier Augustan country ideology. Its basic hierarchical commitment, in fact, led Bolingbroke to repudiate all notions of the state of nature with its egalitarian overtones.¹⁸ But two great historical developments operated to change the context of ideological discourse and most especially among the radicals. The 1760s represent the crucial turning point. The concerns of the earlier part of the century—the mixed constitution, annual Parliaments, the independent Commons, anti-place legislation, and the standing army controversy—were shunted aside. America and the crisis over taxation introduced new noncountry issues into politics. The taxation controversy raised to the center of debate the issue of representation, which in its trail brought to the fore basic concerns about the origins of government and authority in general. Taxation was the curse of all, yet few were enfranchised. Emphasizing taxation flew in the face of ideas of virtual representation and expanded the notion of property beyond landed wealth or freehold. What this emphasis on movable property did, as John Brewer has noted, was to enable radicals like Burgh and Cartwright to extend “the debate about parliamentary reform far beyond its previous confines.”¹⁹ It transcended the paradigms of country ideology to more class-based categories.

¹⁵ Wood, *The Creation of the American Republic*, 21, 23, 36, 47, 56–57, 92, 100.

¹⁶ Bailyn, *The Ideological Origins of the American Revolution*, 283.

¹⁷ This is by no means to take the case as far as Staughton Lynd has in his *Intellectual Origins of American Radicalism* (New York, 1969). He was quite correct in stressing the social content of the English radicals read by Americans during the revolutionary era but quite wrong in reading this content as critical of private property.

¹⁸ Kramnick, *Bolingbroke and His Circle*, 95–106. Also see my “An Augustan Reply to Locke: Bolingbroke on Natural Law and the Origin of Government,” *Political Science Quarterly*, 82 (1967): 571–94.

¹⁹ As should be clear, I share this point of view with John Brewer, who has argued it brilliantly in his *Party Ideology and Popular Politics at the Accession of George III* (Cambridge, 1977), 255. This essay does not address, except in passing, the question of Locke in America during these years. Others, like Joyce Appleby and Ronald Hamowy, are at work questioning republican revisionism on that score. It might be noted here, however, that, if Locke was indeed alive and well in the circles of British reform during these later eighteenth-century years, there must be a high presumption he thrived in America too, if only because writers like Burgh, Price, and



Figure 1: "The Friends of the People." In this Cruickshank caricature (1792), Thomas Paine and Dr. Priestley conspire under diabolical supervision. Note that Priestley sits on gunpowder; a famous speech in which he referred to toppling the religious establishment earned him the nickname "Gunpowder Joe." Reproduced courtesy of the British Library.

The American crisis coincided with a second crucial development, the early years of the Industrial Revolution and the emergence of a new middle-class radicalism. The first decades of industrialization in England saw, as D. E. C. Eversley has calculated, a greatly expanding middling level of English society, families with an income between £50 and £400. This "free, mobile, prudent section of the population" was turning to politics.²⁰ These owners of small and movable property, as well as the new entrepreneurs like Wedgwood and Wilkinson, felt excluded from a political process that affected them daily in their credit transactions, in their tax burden, and in the proliferation of intrusive statute law.²¹

Priestley were the staple reading of Americans in the revolutionary era. See Appleby, "The Social Origins of American Revolutionary Ideology," *Journal of American History*, 64 (1977–78): 935–58, and "Thomas Jefferson and the Interpreters of Nostalgia," *William and Mary Quarterly* (in press); and Hamowy, "Jefferson and Scottish Enlightenment: A Critique of Gary Wills' *Inventing America*," *ibid.*, 36 (1979): 503–23. Also see earlier investigations of the relation between English and American radical thought: Thad W. Tate, "The Social Contract in America, 1774–1787: Revolutionary Theory as a Conservative Instrument," *William and Mary Quarterly*, 22 (1965): 375–91; and Curti, "The Great Mr. Locke, America's Philosopher," 107–51. For the vogue of English radicals in America, see the writings of Bailyn and Wood. Also see Lynd, *Intellectual Origins of American Radicalism*; Jack P. Greene, "Political Mimesis: A Consideration of the Historical and Cultural Roots of Legislative Behavior in the British Colonies in the Eighteenth Century," *AHR*, 75 (1969–70): 337–60; Pauline Maier, *From Resistance to Revolution: Colonial Radicals and the Development of American Opposition to Britain, 1765–1776* (New York, 1972); Oscar Handlin and Mary Handlin, "James Burgh and American Revolutionary Theory," *Proceedings of the Massachusetts Historical Society*, 73 (1963): 38–57; Edward I. Morgan, "Slavery and Freedom: The American Paradox," *Journal of American History*, 59 (1972–73): 5–29; Nicholas Hans, "Franklin, Jefferson, and the English Radicals at the End of the Eighteenth Century," *Proceedings of the American Philosophical Society*, 98 (1954): 406–22; H. Trevor Colburn, *The Lamp of Experience: Whig History and the Intellectual Origins of the American Revolution* (Chapel Hill, 1965); and Colin Bonwick, *English Radicals and the American Revolution* (Chapel Hill, 1977).

²⁰ Eversley, "The Home Market and Economic Growth in England, 1750–1780," in E. C. Jones and G. E. Mingay, eds., *Land, Labour, and Population in the Industrial Revolution: Essays Presented to J. D. Chambers* (London, 1967), 206–59.

²¹ See John Brewer, "English Radicalism in the Age of George III," in Pocock, *Three British Revolutions: 1641, 1688, 1776*, 334–36.

This is not to dismiss out of hand the existence of lingering country content in the radical ideology of Wilkes, Burgh, Cartwright, or Sawbridge. It had been, after all, the ideological reflex of the excluded for a century. Calls for frequent elections and a reformed suffrage along with attacks on placemen were often still uttered in the Machiavellian language of corruption, restoration of first principles, and historical analogies from Roman history. But beneath the familiar surface of the new radicalism that began to emerge during the 1760s were different themes. The new radicalism goes beyond the praise of wise and virtuous landed MPs independent of both the crown and constituent pressure. It goes beyond the Rockingham Whigs' sense that all was well with the political system and that only a change of leadership in which men of virtue replaced wicked men was needed to end "the present discontents." In the new radicalism, there is a new dimension, the conviction that those now excluded—the urban and commercial interests—wanted "in," wanted to be represented in Parliament and wanted their MPs to be their spokesmen, serving their interests, not serving as wise men independent of both court and those who elected them. Thus, in their anger, the new radicals turn on *both* the landed classes and the court-government.

Precisely in this context of a critical shift in the nature and aims of the opposition, Lockean ideas made a dramatic and decisive comeback in the 1760s and 1770s. In Locke far more than in Bolingbroke and his ilk, the unenfranchised middle class and especially the Protestant dissenters found intellectual authority and legitimacy for their radical demands. Locke's ideas reflecting the revolutionary upheavals of the previous century spoke more directly to a Burgh, a Paine, or a Priestley than did the nostalgia of a St. John, a Pope, or a Swift.

The revival of Lockean influence is apparent in the pamphlet that contained the first important call for the reform of Parliament. The anonymous *Reflexions on Representation in Parliament* was published in 1766. Although the pamphlet ostensibly provides a sympathetic reading of James Otis's arguments on taxation and representation, over its defense of the colonies and its plea for reform of the suffrage hovers the spirit of John Locke. The author expounded the principles of the British constitution, offering an idea "of what it was in its original purity." This could well have been the civic humanist quest for first principles or the search for lost Anglo-Saxon rights, but it was instead a search for "principles of truth and reason." At the heart of these principles is the "cession which every man, on entering into civil government makes of some of his natural rights, to enjoy the rest in greater security." On the principles of "equity," the author demanded an "equal representation in Commons." (The Saxon past is cited, but the justification for "equal" representation is the ahistorical "principle of equity.") Representation is "a question of right." The pamphlet ends with a direct invocation of "the celebrated assertions of Mr. Locke's . . . that there remains still inherent in the people a supreme power to remove or alter the legislature, when they find the legislative act contrary to the trust reposed in them; for when such trust is abused it is thereby forfeited and desolves to those who gave it."²²

Locke, whose principles "so favour the natural rights of mankind," is central to

²² *Reflexions on Representation in Parliament* (London, 1766), 4, 6–7.

this opening salvo in the campaign to reform Parliament, and his use here set the pattern for the next thirty years. His notion of contract, of governors as trustees, subject to dismissal if they forfeited this trust, was the intellectual weaponry used in the assault on the unreformed Commons. This is abundantly clear in Wilkes's agitation and its offshoot, the Society of the Supporters of the Bill of Rights. By 1771 the society, led by Horne Tooke, had moved from merely defending Wilkes's right to a seat in Commons to offering a comprehensive program for parliamentary reform. Central to that program was an oath to be required of all parliamentary candidates that they "endeavour to obtain a more fair and equal representation of the people." Echoes of the 1766 pamphlet are clear. Equitable and equal became "fair and equal." John Wilkes moved in the House of Commons on March 21, 1776, "that leave be given to bring in a bill, for a just and equal representation of the people of England in Parliament." In his speech he cited "the present unfair and inadequate state of the representation of the people in Parliament. It has now become so partial and unequal from the lapse of time."²³ The language used in Wilkes's bill is important. For the next thirty years the reform movement used the phrases "fair and equal representation" and "just and equal representation." This abstract language of reason and nature does not derive from specific calculations from the Saxon past, and the principal author of these abstract phrases is none other than John Locke.

In a most striking case of historical oversight, few who have written on this period have noted that this formulation, so central to reform politics and writing for the remainder of the eighteenth century, is lifted directly from Locke, who in paragraphs 157 and 158 of *The Second Treatise of Government* wrote,

It often comes to pass that in government where part of the legislative consists of representatives chosen by the people that by tract of time this representation becomes very unequal and disproportionate to the reasons it was at first established. . . . For it being the interest as well as intention of the people to have a fair and equal representative, whoever brings it nearest to that is an undoubted friend to and establisher of the government and cannot miss the consent and approbation of the community.²⁴

The key phrase of the reform movement was Locke's of nearly one hundred years earlier. Scholars' failure to note the textual derivation from Locke is all the more striking since most eighteenth-century users of the "fair and equal" demand cited Locke as their authority. So it was that Wilkes in his speech of March 21, 1776, noted that "this evil has been complained of by some of the wisest patriots our country has produced. I shall beg leave to give that close reasoner, Mr. Locke's ideas in his own words. . . . [He then read paragraphs 157 and 158.] After so great an authority as that of Mr. Locke, I shall not be treated on this occasion as a mere visionary."²⁵

²³ Wilkes, as quoted in Society for Constitutional Information, *Minutes of a Meeting on Friday 29th of March 1782* (London, 1782), 11–12. For Wilkes, see Christie, *Wilkes, Wyvill, and Reform*; and Brewer, *Party Ideology and Popular Politics*, chap. 9.

²⁴ Locke, *The Second Treatise of Government*, Library of the Liberal Arts (New York, 1952), 89–90. This link between Locke and the reformers in the late eighteenth century is not developed by either J. R. Pole or John Cannon; see Pole's *Political Representation in England and the Origins of the American Republic* (London, 1966), and Cannon's *Parliamentary Reform, 1640–1832* (Cambridge, 1973).

²⁵ Society for Constitutional Information, *Minutes*, 14–15.

Even more important than this textual linkage between Locke and the reformers, however, is the far deeper theoretical bond the reformers constructed between themselves and such Lockean themes as contract, state of nature, and natural rights and government as a trust in all of their writing on taxation and representation. This becomes evident by shifting the focus from Wilkes to more respectable and learned reformers. But Wilkes and his supporters both in London and in the provinces—by and large merchants, manufacturers, and entrepreneurs—forged the link for the enduring character of reform agitation: its antiaristocratic, middle-class bias. Wilkes the fool, Wilkes the court jester, was a living repudiation of hierarchical piety and the due subordination of rank and degree.²⁶

John Cartwright, the grand old man of British reform, illustrates how Lockean ideas, not the more hierarchical views of country ideology, were at the heart of the reform movement. In his *Take Your Choice*, written in 1776, he noted that “the all wise creator hath likewise made men by nature equal as well as free. . . . None are set above others prior to mutual agreement.” Freedom implies choice and equality excludes degrees in freedom. “All the commons have a right to vote in the elections of those who are to be the guardians of their lives and liberties . . . , and no man shall be taxed but with his own consent, given either by himself or his representative in parliament.” The antiaristocratic flavor of the reform movement is apparent in Cartwright, too: “What right has 1/7 of the people who wear laced coats and eat white bread to tell 6/7 who have plain coats and eat brown bread that they have no right to interfere in the election . . . because their want of riches deprived them of many other indulgences, enjoyed by the wearers of laced coats and eaters of white bread?” The source of his arguments is nature, not history. Cartwright insisted that “mankind universally have in all ages had the same unalienable rights to liberty. . . . No charters, exclusions, prescriptions can add to or diminish this right.”²⁷

The middle years of the 1770s saw an outpouring of radical texts in England. The crisis with the colonies did, indeed, raise fundamental questions of authority and obligation, which the civic humanist reading of the period sees as secondary to themes of corruption and virtue. In 1774, for example, there appeared James Burgh’s three-volume *Political Disquisitions*. Burgh ran the influential dissenter academy at Newington Green from 1747 to 1771. There he taught legions of dissenter youth and wrote his huge tome, which schooled generations of British radicals and their American cousins. Samuel Parr, when asked if he had read *Political Disquisitions*, is alleged to have replied, “Have I read my bible, sir?” Much has been made of Burgh’s writings in constructing a case for nostalgic, civic humanist, country ideology at the heart of the reform movement.²⁸ There is, indeed, good reason for this claim. In his three volumes, Burgh often cited Bolingbroke, *Cato’s Letters*, and even Machiavelli. He also wrote at great length about an independent Commons, placemen, corruption, standing armies, annual

²⁶ For this picture of Wilkes, see Brewer, *Party Ideology and Popular Politics*, 197–99.

²⁷ Cartwright, *The Legislative Rights of the Communitary Vindicated: or, Take Your Choice* (London, 1776), 1–2, 31, 27, 116–17.

²⁸ See, in addition to the literature cited in note 19, above, Carla H. Hay, “The Making of a Radical: The Case of James Burgh,” *Journal of British Studies*, 18 (1978–79): 90–118. For the Parr anecdote, see *The Dictionary of National Biography*, s.v. “Burgh, James.”

Parliaments, and lost Saxon rights. But there is much more to Burgh than this simple dependence on mid-eighteenth-century arguments and formulas, and it has generally been overlooked. He is very much in the Lockean individualist tradition, and he injected into the reform movement not only a strong dose of Locke but also a good deal of the bitter, middle-class resentment of the aristocratic quality of the British constitution.

Burgh began and ended his massive work with Locke. At the outset he provided a declaration of his political beliefs that is pure Locke. Authority originates from the people, who, he suggested, receive that power back when their governors betray their trust. When Burgh turned to the inadequate state of representation in Britain early in volume 1, the first authority cited on “the monstrous inequality of parliamentary representation” is Locke. Three volumes later, in concluding his huge tome, Burgh called for a popular association movement to push Parliament to reform. Here he again quoted Locke. Those who accept the trust of governing are answerable to the people, who can refuse obedience and take back power into their own hands. Locke is made the theorist of a popular movement against a corrupt legislature.²⁹

Burgh was no disciple of Bolingbroke, no nostalgic country ideologue defending a gentry-based polity. Parliamentary reform for him formed part of a general assault on aristocratic England:

The landed interest is too well represented, to the detriment of the mercantile and monied. This is an occasion of various evils. For many of our country gentlemen are but bad judges of the importance of the mercantile interest, and do not wisely consult it in their bills and acts. . . . It is the overbalance of the power in the hands of the landed men, that has produced the bounty on exportation of grain which increases the manufacturers' expense of living, and discourages the exportation of our manufactures.

Burgh's attack on placemen, a perennial country opposition issue from the days of Walpole, is very different, however, from Bolingbroke's. Burgh was less incensed with placemen as threats to an independent Commons than he was with them as nonmeritorious, untalented officeholders, who denied the talented middle class access to public positions: “All honours and powers ought to be personal only, and to be given to no individuals, but such as upon scrutiny, were found to be men of such distinguished worth, as to deserve to be raised to distinguished places, though sprung of mean parents.”³⁰

Although now overshadowed by Godwin, Priestley, Price, and Paine, James Burgh through his *Political Disquisitions* was literally the schoolmaster for a whole generation of middle-class radicals in England and America, and it bears repeating that his critique was in large part a self-conscious apology for the assertive middle class. He railed against a British government that parceled out its profitable and prestigious jobs to the nobility and gentry who dominated Parliament. Why has the nobleman, he asked, any more claim to this respect than the artisan or manufacturer? “If the nobility and gentry declined to serve their country in the great offices of

²⁹ Burgh, *Political Disquisitions: An Enquiry into Public Errors, Defects, and Abuses*, 3 vols. (London, 1774): 1: 3–4, 72–73, 2: 279, and 3: 449.

³⁰ *Ibid.*, 1: 51–53, and 2: 89–90.



Figure 2: "Smelling Out a Rat." In this Gilray caricature (1790), Burke, bearing the symbols of church and king, reveals himself to a startled Richard Price. Price was, of course, a distinguished mathematician. Reproduced courtesy of the British Library.

the state, without sordid hire, let the honest bourgeoisie be employed. They will themselves be sufficiently regarded by the honour done them." Rather than "half our nobility . . . and over drenched court sponges . . . being upon the parish" (that is, having public jobs), these jobs, he suggested, should go to men of merit. Burgh proposed, in fact, that public jobs, like public contracts, be filled by "sealed proposals." The talented individual most capable of serving his country would then be selected. If men of the meritorious middle class took over public service and Parliament, then public expenditures, he predicted, would decline dramatically, for these new men would not demand great salaries, they would not dance "at Mrs. Conneley's masquerades." They would "rise up early and sit up late and fill up the whole day with severe labour."³¹

This bourgeois demand for careers open to the talented was a most critical element in British reform during this period. It is Figaro's lament to the great Count Almaviva, translated for Englishmen in 1784 by Godwin's close friend, Thomas Holcroft: "What did you do to deserve what you have—nothing but to be born." It is Tom Paine's suggestion that what "nobility" really means is "no-ability."³² The dissenting schoolmaster James Burgh was neither country apologist nor classical republican. His was an individualist ideology of an insurgent middle class. In America and in Britain, he wrote, the dissenters have rejected the ways of the aristocracy and the poor. They have "bounded" their "riotous appetites" and their "lusts." They have turned their backs on gaming, drunkenness, lewdness, operas, cockfighting, and the theater. Dissenter "sobriety and temperate ways of living," "their thrift and regular manner of living," their awareness "that every

³¹ *Ibid.*, 2: 97–98.

³² Paine, *The Rights of Man*, ed. H. Collins, Penguin Books (London, 1969), 128.

moment of time ought to be put to its proper use,” their “industriousness,” and their “order and regularity” have produced prosperity and wealth. But to what avail is such talent and merit? What are the rewards in an aristocratic society for such achievement, when “the people may be brought, by inveterate tyranny, to bear patiently to see the most worthless part of mankind (for surely the great by mere birth, in all ages and countries are commonly among the most worthless of mankind) set up above them and themselves obliged to crouch”? Sometimes, of course, such “tyranny” becomes too much to bear: “And if the people rouse to vengeance, woe to those who stand in the way. Let merit only be honoured with privilege and prerogative, and mankind will be contented.”³³

Richard Price’s *Observations on the Nature of Civil Liberty*, another critical text in the reform tradition, appeared in 1776. Price did not leave room for doubt about his source, for in his preface he noted that “the principles on which I have argued form the foundation of every state as far as it is free; and are the same with those taught by Mr. Locke.” Government for Price is a trust, in which the people set up governors to serve particular ends. When the trust is betrayed, government is dissolved. Price insisted that the rights of the Americans are the natural rights of all free men, not the product of history, tradition, statute, charter, or precedent. The enemies of the colonists, like Josiah Tucker, condemned the American colonists, Price noted, by calling them “Mr. Locke’s disciples.” “What a glorious title,” Price replied.³⁴

Price’s praise of America provides an interesting insight into the Lockean world view, which so prompted British reformers to excitement when they looked to America. America was, as Locke himself had noted, as it was “in the beginning.” Price and others saw America as a land of individual freedom and equality, where hierarchy and subordination were unknown. The colonies had no rich or poor, he wrote, no beggars and no “haughty grandees.” The Americans were strangers to luxury, and they worked hard. There was no large government, and there were few taxes. Most important of all, Price claimed, in America merit was the only path to distinction.³⁵ To his dying day, Price repeated these themes: in his speech in 1789 and in his *Discourse on the Love of Country*, which so infuriated Burke that he answered with his *Reflections on the Revolution in France*. In the sermon at Old Jewry in 1789, Price alleged that the greatest defect of the British constitution was representational inequality and that its remedy lay in a representational structure that was “fair and equal”—Locke again.³⁶

There are, to be sure, other strains in Richard Price’s work. There is, for example, a deeply pessimistic tone in much of his writings, especially in his repeated fears over the national debt—fears that are also found in the writings of Horne

³³ Burgh, *Political Disquisitions*, 3: 30, 172, 425. Also see his *The Dignity of Human Nature* (London, 1754), 33.

³⁴ Price, *Observations on the Nature of Civil Liberty, the Principles of Government, and the Justice and Policy of the War with America* (London, 1776), ix, 16, 32, 93.

³⁵ Price, *Observations on the Importance of the American Revolution and the Means of Making It a Benefit to the World* (London, 1784), 68–70.

³⁶ Price, *A Discourse on the Love of Our Country Delivered November 4, 1789* (London, 1789), 39.

Tooke and James Burgh and, indeed, in the work of most of the radicals.³⁷ Given as well Price's fascination for the independent farmer in his writings on America, republican scholarship does seem to have a point in insisting that such as Price are not individualist liberals and optimistic modernizers who speak for an insurgent middle class but antimarket nostalgics steeped in civic humanism's "Renaissance pessimism" over the direction of social change and the inevitability of degeneration and decline. In short, the English radicals and their American counterpart Jefferson seem to be direct descendants of Bolingbroke.

There are, however, serious problems with this republican reading. Space here does not permit a full discussion of the rich symbolism of the national debt, and the credit system in general, in Anglo-American political discourse in the eighteenth century. Suffice it to note that by the end of the century criticisms of the debt, of paper money, and of banks and, in turn, praise of independent farmers could not automatically be translated into a politics of nostalgia or a repudiation of capitalism or even of urbanism. Fear of national ruin from an ever-growing national debt was as widespread in the entrepreneurial and manufacturing circles as it was among the middle-class intellectuals in the dissenting chapels and academies. Those who made money in the funds or through the manipulations of the credit system were seen as idle and unproductive. They were part of the immoral, nonindustrious camp that included the nobility, most landed gentlemen, and the nonworking poor. The talented men of the middle class were unknowingly revising the classical, Thomistic dichotomy between a natural and artificial economy. No longer was a subsistence economy "natural" and a market economy for profit "artificial." A natural economy was now characterized by productive hard work and industry, profit notwithstanding; the artificial economy was characterized by idleness and nonproductivity, and its practitioners were the useless aristocrats by birth and the equally useless parasites by profession, the money men who lived off the national debt. Protestant dissenters looked with little favor on the ill-gotten gains of gaming, whether at the table or in the funds.

More important still in accounting for middle-class and modernist sentiment against the debt was its symbolic role as the endless fountain of corruption, the source of jobs and patronage that not only corrupted Parliament but gave society's rewards to the untalented, those without merit. Critical here was the popular identification (well founded in fact) between the growth of the national debt and war. A vast military establishment generated the debt and left the impression in the virtuous, hard-working middle class of an immoral and unholy alliance. All of these came together in the debt. To wage useless wars, to pay for a useless court establishment, to provide jobs and pensions for useless men of no merit there existed a vast debt, which saddled the useful and productive manufacturers and

³⁷ See Price, *Observations on the Importance of the American Revolution*, 60–83, *An Appeal to the Public on the Subject of the National Debt* (London, 1771), *Observations on the Nature of Civil Liberty*, 70–85, 109–30, and *Additional Observations on the Nature and Value of Civil Liberty and the War with America* (London, 1777), 44–46; Tooke, *Causes and Effects of the National Debt and Paper Money on Real and Natural Property in the Present State of Civil Society* (London, 1795); and Burgh, *Political Disquisitions*, e.g., 1: 408, and 2: 298.

artisans with burdensome taxes and high prices. The national debt, moreover, drained capital from industry and raised interest rates and thus was the very symbol of unproductivity and uselessness. The apparent gloom and pessimism of Price and the other reformers has thus been misread. Although Price and Burgh shared a deep Calvinist appreciation of human depravity, they also shared a hatred of the idle. For them, the idle "monied interest" along with idle aristocrats posed a grave threat to the creation of a truly just and moral society in which hard work and productive enterprise are the central values.

It may well be that Price and Burgh were wrong and that those whom they considered immoral and idle funders and bankers would be essential in the creation of the very world they sought.³⁸ It is a mistake, however, to read their views in terms of later economic knowledge. Their opposition to the national debt, the funding system, and the system of paper credit was not reactionary; it went hand in hand with their vision of a moral, middle-class society. Indeed, the national debt enshrined for them much that their new order sought to replace.

It is also true that Price praised America as the home of the independent farmer, and in his writings he warned lest this noble species be overrun by cities, debts, and taxes.³⁹ But, as is the case with Jefferson, this agrarian bias must again not be automatically translated into nostalgia, antimodernism, and anticommercialism, for it is by no means clear that the city was perceived in the late eighteenth century as standing for modernity and capitalism and the countryside for reaction and agrarianism. Yeoman farmers operated very much in the capitalist marketplace and had highly developed commercial networks. The yeoman ideal of both Price and Jefferson was not, as Richard Hofstadter depicted it, "non-commercial, non-pecuniary, self-sufficient."⁴⁰ In defending American agriculture against the Hamiltonian system, Thomas Cooper recognized that encomia for farming did not necessitate a nostalgic repudiation of a commercial society. Although agriculture was a morally superior pursuit, its superiority did not lie in any more virtuous, precapitalist ideal. Commerce had less value only insofar as it drained away resources: "To foster every, or any other employment of capital at the expense of agriculture—by diminishing the savings of the farmer and forcing him to maintain the manufacturer—or by tempting the capitalist from agriculture into manufacture, is plainly contrary to our most undoubted policy."⁴¹

No less committed to a commercial society than others in this period, what distinguishes the economic vision of a Price, a Cooper, a Paine, or a Jefferson is its

³⁸ For an important discussion of the relationship between the entrepreneurial manufacturer and the banking and credit system, see the essays in François Crouzet's *Capital Formation in the Industrial Revolution* (London, 1972). Most manufacturers could decry the national debt and the banking system at this stage in the development of British capitalism because they, in fact, made little use of that system for their capital. Much more characteristic was the self-generation of capital. A good sense of industrial anger with the national debt can be found in Thomas Walker's *A Review of Some of the Political Events which Have Occurred in Manchester during the Last Five Years* (London, 1794). Walker, a wealthy cotton manufacturer and leading dissenter, concluded his book with an attack on the growth of the national debt and "the tremendous burden it makes of additional taxes on the manufacturers."

³⁹ Price, *Observations on the Importance of the American Revolution*, 69–77.

⁴⁰ Hofstadter, *The Age of Reform* (New York, 1955), 23–24.

⁴¹ Cooper, ed., *Emporium of the Arts and Sciences*, new ser., vol. 1, no. 1 (1813): 8.

individualistic, decentralized, and nonhierarchical flavor. Thus, Jefferson preached the virtues of unrestrained free trade in terms of an idealized, individualistic marketplace. "Our interest will be to throw open the doors of commerce, and to knock off all its shackles," he wrote, "giving perfect freedom to all persons for the vent of whatever they may choose to bring into our ports, and asking the same in theirs." So, too, Price praised America because, unlike "older countries" where rural life was graduated into ranks of "gentry, yeomanry and peasant," America had just yeomen, "all independent and nearly upon a level."⁴² The market was not the villain, hierarchy and dependence were. As Joyce Appleby has noted of Jefferson, "what was distinctive about the Republican's economic policy was not an anti-commercial bias, but a commitment to growth through the unimpeded exertions of individuals" with "access to economic opportunity."⁴³

The rural-urban dichotomy and the preference for the rural is compatible with the emerging middle-class vision. The countryside (where, after all, in England the early manufacturing occurred) represented hard work, simplicity, frugality, industry, and productivity. The city represented courts, office holders, pensioners, luxury, waste, money, and funds. In the city congregated the idle, either the very rich or the very poor. In the city were gaming, opera, theater, and other useless, time-wasting activities. To label the city corrupt and the countryside virtuous need not, then, immediately connote a dread of modernity. The ideological thrust of such activities is never simply read. Who is virtuous and who is corrupt is not reducible to who is engaged in agriculture and who in commerce, who lives on the land or who on city streets. Hard work, talent, and productivity are what are really critical in the distribution of moral worth, and the secondary distinctions based on geography have to be read in light of these much more crucial, more primary issues.

Joseph Priestley is a case in point. He did, to be sure, become involved in the Pantisocracy, a scheme with Coleridge for a rural utopia in America, and he finally settled in the Pennsylvania back country, near Northumberland. Yet no more zealous a modernizer and liberal apologist for the middle class can be found in the ranks of British reform in these years. More than anyone else, he qualifies as the central intellectual figure among these middle-class radicals. Radical in politics, laissez faire theorist in economics, innovator in science and technology, founder of the modern Unitarian movement, Priestley schooled England's new men of

⁴² Jefferson, *Notes on the State of Virginia*, ed. W. Peden (Chapel Hill, 1955), 174; and Price, *Observations on the Nature of Civil Liberty*, 70–71.

⁴³ Appleby, "Thomas Jefferson and the Interpreters of Nostalgia," *William and Mary Quarterly* (in press). Price's life was that of urban preacher and intellectual. He was intimately connected with the dissenter industrialist community, and as a mathematician his work in actuarial science became the basis of that most commercial of enterprises, the life insurance industry. Jefferson was not above having labor-saving machines and manufacturing mills at Monticello. Indeed, as Merrill Peterson has noted, "Here was no pastoral Eden but belching smoke and clanging hammers"; Peterson, *Thomas Jefferson and the New Nation: A Biography* (New York, 1970), 535–36. Industry and manufacture on a small scale were clearly compatible with a virtuous life of hard work and productivity. For a late-eighteenth-century argument that factories and manufacturing contribute "to paths of virtue, by restoring frugality and industry" while counteracting aristocratic corruption and luxury, see Trench Coxe, "An Address to an Assembly of the Friends of American Manufacturers," *American Museum*, 2 (1787): 251, 253–55.

business in the series of dissenting academies at which he taught, while personally serving as the critical link between virtually every aspect of the progressive liberal nexus. Brother-in-law to Wilkinson, friend of Price and Wollstonecraft, “guide, philosopher, and friend of Boulton, Watt, and Wedgwood at Birmingham,” he was “gunpowder Joe” to Burke and the Church-and-King mob that burned his laboratory and home in 1791, sending him to finish his days in the reformer’s paradise—America.⁴⁴

Even more than Price, Joseph Priestley used Locke in the 1770s. *An Essay on the First Principle of Government* (1771) is virtually a gloss on *The Second Treatise*. Priestley began by describing a state of nature filled with people “living independent and unconnected,” who “voluntarily resign some part of their natural liberty” to magistrates who then are the people’s servants. These magistrates are accountable to the people, and, if they abuse the trust given them, they can be deposed. Punishing their governors is a natural right of the people. So keen on arguments from nature was Priestley that he dismissed out of hand those who cited historical rights, specifically lost Saxon rights. Saxon England, Priestley argued, was a time of idleness, treachery, cruelty, and insecurity of property. It was no golden age or moment of pure principle that should be reinstated.⁴⁵ In his economic thought, Priestley offered a vigorous defense of laissez faire and individualistic attacks on the poor laws. He also attacked the national debt. But, again, this was the politics not of nostalgia but of a modernizing middle class. He complained that, among other things, the growing debt raised prices. Particularly distressing was the rising price of bread, for, “by keeping bread at a reasonable price, workmen’s wages are kept lower, and more fixed, a thing of the greatest consequence in manufactures.”⁴⁶

The spirit of John Locke hovers over Priestley’s writings on religious individualism, which so influenced Jefferson and which are directly derivative of Locke’s *Letters on Toleration* and, in turn, of Locke’s views of government in *The Second Treatise*. Government, Priestley wrote, should not be involved in the religious beliefs of citizens:

The magistrate’s concern is not with opinions and beliefs. His proper duty is to preserve the peace of society, or to see that no member of it injures another man in his person or his property. . . . How is any person injured in these respects by my holding religious opinions of which he disapproves? . . . If you say it will endanger the salvation of others what does that matter to the magistrate whose business is with the things of this life only, who was not appointed to act any part in things of a spiritual but only in those of a temporal nature.⁴⁷

Once again Locke lives, for it was clear in the late eighteenth century that behind such ideas as these stood, among others, Locke. In America a well-known cartoon

⁴⁴ Robert E. Schofield, *The Lunar Society of Birmingham* (Oxford, 1963), 353.

⁴⁵ Priestley, *An Essay on the First Principles of Government and on the Nature of Political, Civil, and Religious Liberty, Including Remarks on Dr. Brown’s Code of Education* (London, 1771), 2–7, and *Lectures on History and General Policy* (London, 1788), 349.

⁴⁶ Priestley, *Lectures on History and General Policy*, 394. For his critical views of the poor and the poor laws, see *ibid.*, 295–305, and *An Account of a Society for Encouraging the Industrious Poor to which Are Prefixed Some Considerations on the State of the Poor in General* (Birmingham, 1787).

⁴⁷ Priestley, *Conduct to Be Observed by Dissenters in Order to Procure the Repeal of the Corporation and Test Acts* (Birmingham, 1789), 6–7.



Figure 3: Dr. Joseph Priestley, Unitarian minister, educator, distinguished scientist, and eminent disciple of John Locke. Priestley is buried in Northumberland, Pennsylvania. Portrait reproduced courtesy of the British Museum.

of 1769 labeled “An Attempt to Land a Bishop in America” shows the bishop hastily sailing back to England. At the departing cleric, the angry crowd is throwing epithets labeled “Calvin’s Works,” “liberty and freedom of conscience,” and “Locke.”⁴⁸

Locke also flourished in the two major organizational expressions of British reform in these years, the Society for Constitutional Information (SCI) of the 1780s and the London Corresponding Society (LCS) of the 1790s. Founded in the early 1780s, the SCI circulated to its members excerpts from books, speeches, and

⁴⁸ For a full description of this cartoon, see Herbert D. Foster, “International Calvinism through Locke and the Revolution of 1688,” *AHR*, 32 (1926–27): 476.

pamphlets that the society's leaders—Horne Tooke, Capel Lofft, Christopher Wyvill, John Jebb, and John Cartwright—felt argued for reform of Parliament or more generally praised freedom. Here, as in Burgh's *Political Disquisitions*, much use was indeed made of Bolingbroke, *Cato's Letters*, and the whole arsenal of country opposition to Walpole. There is even occasional talk of Machiavellian "ritorno ai principii," signifying a return to the lost rights and purer principles of Saxon times. The SCI leader most likely to use this older country language was Christopher Wyvill. His letters and political papers are full of pleas to the other reformers to back Pitt's more moderate reform bills, to avoid plans of "theoretical perfection" or schemes "to form a government on a perfect theory." He spoke less of natural than of Saxon rights, and he spent a good deal of energy in the 1790s criticizing the abstractions and ahistorical arguments of Thomas Paine.⁴⁹

But alongside Wyvill's country ideology and civic humanism spoke another and more dominant voice in the SCI, the voice of Locke, of natural rights, of compact, of government as trust, of natural equality, and of the people's power to change governments. John Jebb, the Peterhouse Anglican who converted to Priestley's Unitarianism, rejected Wyvill's expectation that Parliament would reform itself. It was too subject to the will of the prime minister. Better for the people themselves in public association and assembly to reassert their natural right to change governments. This was their residual right from the original compact. Only by popular pressure outside Parliament, Jebb insisted, could the fair and equal representation that Wilkes sought be realized.⁵⁰

Capel Lofft, another major figure in the SCI leadership, offered a gloss on Locke in 1779, applying the principles of natural law and the social compact to the reform of Parliament. All men were equal "by the law of nature," and the power of governors derives "only from consent and contract." Neither Saxon nor seventeenth-century commonwealth history are the sources of Lofft's radicalism: "As to our liberty, we derived it not, if we mean the right from our forefathers or from the Revolution; we had it from God, from the nature of man, and the nature and ends of society. We respect human authority so far as it is founded in public consent and directed by that principle to public utility." In the 1790s Lofft repeated the superior claim of natural rights over historical rights. The former "is of date far higher, and of origin transcendently more venerable. It is an inheritance coeval with the commencement of humanity." In general, Lofft defended the dissenters from Burke's wrath, and he invoked the familiar cry that no longer should "those of useful industry" be barred from "public counsel." It was time, he urged, to break down the barrier "separating the useful from the honored classes in the Community." The "temples of honour" were open only to the "mere presumptions of merit." Reformers meant, he wrote, to "expand the gates and enlarge the avenues" to these temples.⁵¹

⁴⁹ Wyvill, *Political Papers*, 4 vols. (London, 1794), 2: 605, 3: Appendix, 154, and 4: 75–90.

⁵⁰ Jebb, *The Works of John Jebb*, 3 (London, 1787): 180, 455–84. Also see Jebb to Wyvill, August 7, 1781, in Wyvill's *Political Papers*, 4: 497–521.

⁵¹ Lofft, *Elements of Universal Law and Particularly of the Laws of England* (London, 1779), 10–15, *Observations on Mr. Wesley's Second Calm Address* (London, 1777), 55, and *Remarks on the Letter of the Rt. Honourable Edmund Burke concerning the Revolution in France* (London, 1790), 3, 31, 35.

The Butterfield-Christie reading of English reform in the late eighteenth century, so crucial for republican scholarship, needs rethinking. Far from being principally backward-looking seekers of lost Saxon rights, uninterested in socio-economic themes, those who called for the reform of Parliament in the late eighteenth century were, in fact, very much preoccupied with the social question. Moreover, in the SCI documents, lost Saxon rights were read as natural rights. The purpose of the Saxon constitution, according to the SCI leadership, was to preserve natural rights. To re-establish the Saxon constitution, then, was seen as recapturing inalienable natural rights of free men. This is a very different emphasis from that found in Bolingbroke and his circle. Most of the pamphlets circulated by the SCI affirm that reason and nature, not custom and history, are the points of reference. The dictates of reason and nature have all men free, possessing absolute rights, from the “immutable laws of nature.” Among these rights are the right to be taxed only through consent and the right to “fair and equal representation.” Here, too, in the SCI America was held up as the beacon for a corrupt Britain, and not because it represented a repudiation of modernity but because it stood behind the rights of nature, of equity, and of reason.⁵²

Locke is quoted extensively in SCI documents on the people’s responsibility when their natural rights are violated. The SCI’s pamphlets never tire of describing governors as trustees who may be turned out when they have violated the people’s trust. He was the source of the judgment that the people could remove legislators who violated that trust. The people have a recourse for a corrupt, despotic House of Commons. They have “a just and natural control” and a right “to a just resistance . . . to an invasion of their natural and inalienable rights.”⁵³ In the SCI pamphlet of 1780, *The Letter of Lord Carysfort on Parliamentary Reform*, this Lockean theory is called down to the world of practice:

Mr. Locke is of the opinion, that “there remains inherent in the people a power to remove or alter the legislative, when they find the legislative act contrary to the trust reposed in them; for when such a trust is abused, it is thereby forfeited, and dissolves to those who gave it.” If this conclusion is just in theory, it must be just in practice; and . . . may be adopted and argued from under the present dispensation of Government.⁵⁴

This translation of Locke from theory to practice was even more apparent in the London Corresponding Society. This artisan-based, more radical group of the 1790s also shows the unmistakable stamp of John Locke. The call to establish the LCS in 1792 describes how men voluntarily give up some of their rights better to secure the possession of others. Among the rights an individual retains is “the right to share in the government of that society of which he is a member.” To insure that right, Thomas Hardy, the secretary of the society, advocated “a fair, equal and

⁵² Society for Constitutional Information, *Minutes of the Meetings . . . Held at No. 2 in New-Inn* (London, 1782), 1–30.

⁵³ *Ibid.*, 37. Also see Society for Constitutional Information, *An Address to the Public . . .* (London, 1780), 1–8, and *A Second Address to the Public . . .* (London, 1780), 1–15.

⁵⁴ Society for Constitutional Information, *A Letter from the Rt. Honourable Lord Carysfort to the Huntingdonshire Committee* (London, 1780), 4–5.

impartial representation." Like the SCI, the LCS was able to cite as the source of these rights both "our Saxon inheritance" and "our inheritance from nature and the immutable principles of justice." But much more typical of these friends of liberty and ideological kinsmen of Tom Paine was the invocation of nature.⁵⁵

It is important to note the imprint of Locke on the LCS, for much has been made of the LCS recently by E. P. Thompson as an example of early working-class ideology in action.⁵⁶ Radical the members of the society were, granted, but radical petty bourgeoisie. Their *Declaration of Principles* is a fascinating document, very much influenced by the spirit of Locke. Principle Number One proclaims "that all men are by nature free, equal and independent of each other." No reference is made to Locke, but this passage is, of course, taken verbatim from the opening of paragraph 95 of *The Second Treatise*. Principle Number Two states that, "to enjoy all the advantages of civil society, individuals ought not to relinquish any more of their natural independence than is necessary to preserve the weak against the strong, and to enable the whole body to act with union." The LCS *Declaration of Principles* also includes a listing of the civil rights of every individual. At the top is "equality of protection for his liberty, life and property." Further down another important civic right is claimed, one that is by now familiar as a critical issue for these reformers. It is "equality of encouragement for the exercise of his talents, and consequently the free enjoyment of the advantages thereby obtained."⁵⁷

The LCS pamphlets and manifestoes abound with references to the membership as "taxpaying, industrious and useful inhabitants" or "industrious and worthy citizenry." The society's world view, like that of the reform movement in general, saw two classes in British society, the useful and the useless:

We take pride in acknowledging ourselves a part of that useful class of citizens which placemen (pensioned with the extorted produce of our daily labour) and proud nobility wallowing in riches (acquired somehow) affect to treat with a contempt too degrading for human nature to bear, unless reconciled to it by the reflection that though their inferiors in rank and fortune, we equal them in talents and excel them in Honesty.

The LCS was by no means an enemy of private property. It acknowledged "that differences of strength, of talents, and of industry, ought to afford proportional distinctions of property which when acquired and confirmed by the laws, is sacred and inviolable."⁵⁸

When these talented and industrious citizens convened in a huge open-air meeting at Chalk Farm on April 14, 1794, they knew full well the mood of Pitt's government and the growing evidence of repression. Their unanimous resolution, critical of the government, was a paraphrase of Locke. It was sound constitutional doctrine, to be sure, but in the context of the mid-1790s even the spirit of Locke was suspect. The LCS resolved

⁵⁵ London Corresponding Society, *The . . . Society to the Nation at Large* (London, 1792), 1–2, 6, and *An Account of the Proceedings of the British Convention Held in Edinburgh, the 19th of November 1793* (London, n.d.), 10–12.

⁵⁶ Thompson, *The Making of the English Working Class* (New York, 1963), esp. 152–57.

⁵⁷ London Corresponding Society, *Report of the Committee Appointed to Revise and Abridge a Former Report of the Constitution of the . . . Society* (London, n.d.), 1, 2.

⁵⁸ London Corresponding Society, *Addresses and Resolutions* (London, n.d.), 1, *Address of the . . . Society to Other Societies of Great Britain United for Obtaining a Reform in Parliament* (London, 1793), 8, and *Address from the . . . Society to the Inhabitants of Great Britain on the Subject of a Parliamentary Reform* (London, 1792), 2–3.

that any attempt to violate those yet remaining laws, which were intended for the Security of Englishmen against the Corruption of dependent judges ought to be considered as dissolving entirely the social compact between the English nation and their Governors, and driving them to that incontrovertible maxim of eternal Justice, that the safety of the people is the supreme, and in cases of necessity, the only law.⁵⁹

WHAT BETTER PROOF COULD THERE BE of Locke's importance in the late eighteenth century than a crusade to root out his ideas waged by the opponents of reform? So common was the reformers' use of Locke's "close reasoning" that their Tory and clerical opponents often singled out Locke as the sinister influence behind radical agitation. Richard Hey and Bishop William Paley ridiculed the reformers'—that is, Lockean—notions of contract, the state of nature, and natural rights.⁶⁰ The *Anti-Jacobin* lamented that "Price, Priestley, Rousseau, Paine could justify on the principles of Locke, their own visionary doctrines, pregnant with consequences so mischievous to society and so different from what Locke himself intended."⁶¹

George Horne, the bishop of Norwich and chaplain to George III, criticized Locke's notion of natural equality and independence in the state of nature, "since from the beginning, some were born subject to others." Samuel Horsley, bishop successively of St. David, Rochester, and St. Asaph, chimed in that Locke's description of an original compact and an independent state of nature were "absurd and unphilosophical creations of something out of nothing." In his annual sermon marking the anniversary of the decollation of Charles I, Horsley linked together in unholy alliance Lockean doctrine, Protestant dissenters, and French Jacobins. "Contractual ideas of popular sovereignty inflamed the phrensy of that fanatical banditti," and the "dissemination of those infernal maxims that kings are the servants of the people, punishable by their masters" has led now to the murder of Louis XVI. Behind it all, Horsley lamented, is English seventeenth-century doctrine.⁶²

The most vocal denouncers of Locke in this period, however, were Josiah Tucker, the dean of Gloucester, and Edward Tatham, an Anglican divine from Oxford. Tucker's *Treatise Concerning Civil Government* (1781) is one long diatribe against Locke and those whom Tucker called his "eminent disciples"—Priestley and Price. What particularly upset Tucker about Locke, Price, and Priestley was their conviction "that government is a work of art, and that nature has no share in forming it, in predisposing or inclining mankind to it." He repudiated their notion that men were ever "independent and unconnected beings" who voluntarily chose to set up governments. He also rejected what he described as their conviction that "civil government is a necessary evil, rather than a positive good." What folly, he

⁵⁹ *The Annual Register* (London, 1794), 264.

⁶⁰ See Hey, *Happiness and Rights: A Dissertation upon Several Subjects Relative to the Rights of Man and His Happiness* (York, 1792); and Paley, "Elements of Political Knowledge," in *The Collected Works of William Paley*, 3 (London, 1838): bk. 6.

⁶¹ *Anti-Jacobin*, as quoted in Anthony Lincoln, *Some Political and Social Ideas of English Dissent, 1763–1800* (Cambridge, 1938), 114.

⁶² Horne, *Discourses on the Origins of Civil Government* (London, 1800), 271; and Horsley, *A Sermon Preached before the Lords Spiritual and Temporal . . . , January 30, 1793* (London, 1793), 4, 22–23.



Figure 4: "Dr. Phlogiston" (1791), unattributed. This anti-Priestley cartoon was published in the same year that his laboratory and home were burned in Birmingham by a Church and King mob. Note the titles of his manuscripts: "Political Sermon," "Essay on Government," "Essays on Matter and Spirit," "Gunpowder," and "Revolution Trusts." Reproduced courtesy of the Birmingham Public Library.

wrote, to describe men as “all equal, all free, and independent, all masters of self.” Against Locke and his late eighteenth-century radical disciples Tucker invoked Aristotle, who, Tucker insisted, had correctly described the inherent sociability of human beings and the naturalness of the political community. Tucker was particularly angered by the use of Locke in America, for the Americans had taken to heart his pernicious doctrines and would create a “Lockean Republic, where all taxes are to be free gifts! and every man is to obey no further, and no otherwise, than he himself chooses to obey.”⁶³

Edward Tatham in 1791 continued the assault on Locke and those he called “the two captains” of his teachings in England, Price and Priestley. And Tatham had the same alternative to Locke. Priestley should have read more Aristotle and less Locke, Tatham suggested. The Aristotle Tatham invoked is not, however, Aristotle the theorist of citizenship and the *zōon politikon* so dear to twentieth-century republican scholarship but Aristotle the theorist of hierarchy and privilege. Aristotle, Tatham wrote, “took men as he found them and as history informed him they have been always found, connected in society, subordinate and dependent on each other.” Against Locke, Price, and Priestley, Tatham preferred an Aristotle who taught “that men are made by their Creator different and unequal, some formed for authority, and others for subjection.” Locke and his eighteenth-century captains, Tatham insisted, would “throw down all ranks and distinctions of man.”⁶⁴

Late-eighteenth-century observers thus made a clear link from Locke to British reform and socioeconomic change, a link that has been denied by the “Republican School.” The real threat of Locke and his “eminent disciples” was their leveling tendencies, their assault on traditional aristocratic society. In *The True Basis of Civil Government in Opposition to the System of Mr. Locke and His Followers*, Tucker criticized the reformers for denying what he saw as basic in human nature: “a certain ascendancy in some, a kind of submissive acquiescence in others.” There are, Tucker insisted, certain natural “ranks in society” and “stations in life,” which contract theory undermines in its subversive preoccupation with natural freedom and equality. These critics perceptively understood the intentions of the “eminent disciples” of Locke. The praise of achievement and talent, the ideology of equal opportunity, and the cult of industry and productivity, all wrapped in doctrines of natural equality and independence, were in fact self-consciously directed at “the age of chivalry.” The true nostalgics were the likes of Burke, Tucker, and Tatham. Theirs was the defense of a world with “Kings, princes, nobles and gentlemen. . . . For in the whole scale of beings, and in the nature of things, there must be regular gradations and regular distinctions.”⁶⁵

Less concerned with nostalgia than with repression, Pitt’s government brought numerous leaders of the reform movement to trial for treason. Even above the trials hovered the spirit of John Locke. So subversive were his ideas considered that

⁶³ Tucker, *Selections from His Economic and Political Writings*, ed. R. L. Schuyler (New York, 1931), 407–553 *passim*, esp. 464.

⁶⁴ Tatham, *Letters to Edmund Burke on Politics* (Oxford, 1791), 7, 12, 22, 26.

⁶⁵ Tucker, *Economic and Political Writings*, 477; and Matthew Goodenough, *Plain Thoughts by a Plain Citizen of London* (London, 1792), 11.

to be thought a Lockean in the 1790s was itself grounds for suspicion. According to Priestley, "any sentiment in favour of liberty that is at all bold and manly, such as till of late, was deemed becoming Englishmen and the disciples of Mr. Locke, is now reprobated as seditious."⁶⁶ No surprise, then, that sympathy with Locke's ideas was raised as an issue in the trials of Hardy, Gerald, and Paine.⁶⁷

Despite these attacks, Locke had his proud defenders. Thomas Erskine, the eloquent defense attorney for so many of Pitt's victims, still invoked Locke as a warning to the government. "It is justly observed by Locke," he declared, that "after much neglect and provocation the people will be roused to a reasonable and justifiable resistance." Robert Hall, the dissenting minister, was unafraid to praise Locke in his published reply to Bishop Horsley's anniversary sermon. Hall proudly proclaimed that the doctrine of Locke and his followers "is founded on the natural equality of mankind; for as no man can have any natural or inherent right to rule any more than another, it necessarily follows, that a claim to dominion, wherever it is lodged, must be ultimately referred back to the explicit or implied consent of the people." The "immortal Locke" is, indeed, the patron of English reform.⁶⁸

Joseph Towers, the dissenting minister in Highgate and a friend of Price and Priestley, wrote *A Vindication of the Political Principles of Mr. Locke*, in which he praised the reformers' use of Locke, describing him as "an ornament and an honour to the country which gave him birth." Towers met the historical objections to Locke head on, insisting that the "great excellence of his maxims" is that they are based not "upon the iniquities of a dark and intricate and disputable nature" but upon the more indisputable "principles of reason and justice . . . apparent at all times." As for objections to Locke's doctrine of changing governors who act "contrary to the trust given them," Towers responded, "You might as well say, honest men may not oppose robbers or pirates, because it may occasion disorder or bloodshed." Towers even wrapped himself in Locke when demanding the suffrage for Birmingham, Manchester, and Leeds. Had Locke not begun it all, Towers asked, with the passage on "bringing Parliament to a fair and equal representation"? Far from denying Locke's role, the Presbyterian Towers praised it: "It may be readily admitted, that Mr. Locke and his followers wish to extend the present partial representation of the people, and to make it more agreeable to reason and to justice . . . , but this is not one of the defects of Mr. Locke's system, but one of its principal excellencies."⁶⁹

Even more telling than Towers's *Vindication* was the pamphlet of 1794, *The Spirit of John Locke*, written by Henry Yorke of Sheffield, who in that same year was tried for treason.⁷⁰ The pamphlet, widely distributed to all radical associations by the

⁶⁶ Priestley, *An Appeal to the Public on the Subject of the Riots in Birmingham* (Birmingham, 1792), 113–14.

⁶⁷ Thompson, *The Making of the English Working Class*, 88–89, 128–29; F. D. Cartwright, ed., *The Life and Correspondence of Major Cartwright*, 1 (London, 1826): 214–17; and Lloyd Paul Stryker, *For the Defense—Thomas Erskine* (Garden City, N.Y., 1947), 224.

⁶⁸ Erskine, *A View of the Causes and Consequences of the Present War with France* (London, 1797), 22; and Hall, *An Apology for the Freedom of the Press and for General Liberty, to Which Are Prefixed Remarks on Bishop Horsley's Sermon* (London, 1793), 40, 72, 62.

⁶⁹ Towers, *A Vindication of the Political Principles of Mr. Locke* (London, 1782), 13, 16, 40, 65.

⁷⁰ For Yorke's fascinating career, see J. Taylor, "The Sheffield Constitutional Society," *Transactions of the Hunter Archaeological Society* (Sheffield), 5 (1943): 133–46; and G. P. Jones, "The Political Reform Movement in Sheffield," *ibid.*, 4 (1937): 57–68. Later in his career (after a long prison term), Yorke moved to the right and published an attack on his former radical colleagues.

Constitutional Society of Sheffield, was a popular abridgment of *The Second Treatise of Government* designed to bring Locke's ideas more readily to "that part of the community who have not leisure to employ themselves in perusing the whole of the work." The abridged Locke, Yorke wrote, was

applicable to the present times.—It may serve to open the eyes of our deluded Countrymen, who are persecuting and hating us, because we are vindicating the ancient Liberties of our Country. It will expose the fallacious reasoning of those who would persuade the People that they have no other rights but what their rulers please to give them. It will prove passive obedience to be folly, and RESISTANCE AGAINST OPPRESSION to be the duty of the people.

Locke's credentials were good, Yorke informed the reader, and his ideas had served well in the recent American "cause" so sacred to the dissenters and the reformers:

While America was gloriously struggling to throw off the yoke of British oppression, the name of John Locke resounded even in the British Senate, and was echoed in Congress. The principles, which he had so admirably digested from amidst the crude volumes of remoter times, were, to oppressed Humanity, the alarum of Liberty. They taught the criteria of good and bad Government, and instructed the injured how, and when, to trample Despotism under foot.

Locke's writings might be dull, Yorke conceded, but Locke was relevant and his spirit alive. Speaking for his reforming colleagues in Sheffield, he commended Locke to readers who were less familiar with the "close reasoner" than they:

We commit to your wisdom and prudential reflection, the following Abstract from a book, difficult to be purchased, and, throughout the greatest part, uninteresting even when purchased. We have declined making any notes to the body of the work. But he who runs may read, and the Man who cannot take a broad hint from the *last* century, must not expect one from the *present*.

In trying to make the strongest case he could for knowledge of Locke, Yorke noted that Edmund Burke, "Knight Errant of Feudality," had claimed on the floor of the House of Commons that "Locke's *Treatise on Civil Government*, was the worst book ever written." Yorke was therefore "certain it needs no farther recommendation."⁷¹

TO PARAPHRASE MARK TWAIN, the scholarly consensus on Locke's death in the late eighteenth century is greatly exaggerated. Late eighteenth-century English reformers dramatically worked Lockean themes into the heart of their critique of traditional England, turning to Locke because *The Second Treatise of Government* was uniquely appropriate to their peculiar problem. Parliament, the representative body, was itself the barrier to reform. Locke's belief in the residual power of the people against their governors legitimized the reformers' campaign against the unreformed House of Commons. Locke's political theory legitimated their demands, both substantive and procedural, for a reform of the suffrage and of

⁷¹ Yorke, *The Spirit of John Locke on Civil Government* (Sheffield, 1794), iii–iv, viii.



Figure 5: "The Contrast" (1792), unattributed. The critics of the radicals were quite specific about what horrors Jacobinism would bring to England. Reproduced from the author's collection.

parliamentary representation. His concept of a limited secular magistrate legitimated their demand for the separation of church and state.

The reformers also turned to Locke because their ideological concerns were similar. These spokesmen for an insurgent middle-class radicalism were drawn to this liberal theorist of possessive individualism. No one had better expressed their economic and social convictions than Locke had. His socioeconomic vision was perfectly compatible with—indeed, had helped shape—their image of a world peopled by hard-working, industrious property owners. Locke had often written on the themes of industry and talent and was perceived as a crucial part of the Protestant tradition that so informed much of this reform movement. In the libraries of the dissenting academies, Locke's works were standard references, not only for psychology and education but also for politics and commerce.⁷² Had not, in fact, *The Second Treatise* contended that God had set some men to be more industrious than others and thus to acquire more property? Locke's conviction that God "gave the world to the use of the industrious and Rational . . . , not to the Fancy or the Covetousness of the Quarrelsome and Contentious," was critical to his thought; it symbolized a central tension in his work: the struggle between the industrious and the idle—a struggle at the core of the world view of those late eighteenth-century dissenting reformers who read Locke in the libraries of their academies.⁷³

The praise of industry, of what the seventeenth and eighteenth centuries considered "skill, assiduity, perseverance and diligence," and the denunciation of idleness were, of course, by no means unique to Locke.⁷⁴ Protestant writers, especially Puritans like Baxter, had long made them a crucial part of their notions of work, of the obligation to labor, and of the importance of one's calling. What Locke did was wed these earlier views to a political theory of private rights and individualism with his argument that property was an extension of self, the injection of personality into nature through work.⁷⁵ Less apparent, however, is the extent to which other Lockean texts read in these dissenting academy libraries spoke to the themes of industry, idleness, and the glory of work. Indeed, so concerned with these themes was Locke that it is little exaggeration to suggest that he saw industriousness as the central characteristic of the human personality, of personal behavior, and of social and personal activity.

In his *Essay Concerning Human Understanding*, Locke linked activity to anxiety, a connection not unfamiliar to later readers of Weber and Tawney. "The chief, if not the only spur to human industry and action is," he wrote, "uneasiness." This feeling of uneasiness, a desire for "some absent good," drove men to enterprise and unrelenting activity. Once motivated, they were permanently active, for they had a

⁷² Lincoln, *Some Political and Social Ideas of English Dissent, 1763–1800*, 87.

⁷³ Locke, *The Second Treatise of Government*, chap. 5, secs. 48, 35.

⁷⁴ For a discussion of the word "industry," see Raymond Williams, *Culture and Society, 1780–1950* (London, 1958), 13.

⁷⁵ The following argument owes a good deal to the interesting work of E. J. Hundert. See his "The Making of *Homo Faber*: John Locke—Between Ideology and History," *Journal of the History of Ideas*, 33 (1972): 3–22, and "Market Society and Meaning in Locke's Political Philosophy," *Journal of the History of Philosophy*, 15 (1977): 33–44.

never-ending “itch after honour, power and riches,” which in turn unleashed more “fantastical uneasiness.”⁷⁶

Activity and industry, according to Locke, also characterized childhood. His widely read *Thoughts Concerning Education* is a veritable diatribe against idleness. Children were the model for the species. They “generally hate to be idle. All they care then is, that their busy humour should be constantly employ’d in something of use to them.” All life is industrious activity, Locke wrote; even in recreation human beings are never idle. “For *Recreation* is not being idle (as every one may observe) but easing the wearied Part by Change of Business: and he that thinks *Diversion* may not lie in hard and painful Labour, forgets the early Rising, hard Riding, Heat, Cold and Hunger of Huntsmen, which is yet known to be the constant Recreation of Men of the greatest Condition.” Recreation should not just delight and provide ease, it should refresh one for “regular business” and “produce what will afterwards be profitable.” Uneasiness again preoccupied Locke, as he prescribed that parents keep their children busy and fight “the dead weight of unemployed Time lying upon their hands,” since “the uneasiness it is to do nothing at all.”⁷⁷

Children should be taught to keep account books, according to Locke, which not only would keep them busy but would also teach them frugality. Such practices would contribute to the habitual and orderly management of their lives. Industriousness required that children also learn to postpone immediate gratification: “He that has not a mastery over his Inclinations, he that knows not how to *resist* the Importunity of *present Pleasure* or *Pain*, for the sake of what Reason tells him is fit to be done, wants the true Principle of Virtue and Industry.”⁷⁸

Two groups in the community—some aristocrats and all of the poor—were not, however, active and industrious. According to Locke, they provided the “fancy or the covetousness of the Quarrelsome and contentious.” As Burgh, Price, Priestley, and all dissenting middle-class reformers eventually did, Locke divided society into an industrious, enterprising middle beset by two idle extremes. In his essay of 1691 on lowering the interest rate, Locke criticized the profligate aristocrat, whose plight was produced by “debauchery and luxury beyond means.” He was not criticizing the “industrious and rational” among the aristocracy, only the “covetous” and “contentious,” who “will have and by his example make it fashionable to have more claret, spice and silk.” Such men would soon lose their power and authority to “men of lower condition who surpass them in knowledge.”⁷⁹

While only some of the aristocracy were lazy and “debauched,” Locke presumed that most of the poor were idle and inactive, and his harshness toward them knew no bounds. He urged that the provisions of the poor law be made stricter so that the poor could be taught industry, hard work, and frugality. One way to break them of their wasteful idleness was through the establishment of what Locke called

⁷⁶ Locke, *An Essay Concerning Human Understanding*, ed. A. C. Fraser, 2 (Oxford, 1894): bk. 20, sec. 6.

⁷⁷ Locke, *Some Thoughts Concerning Education* (Cambridge, 1892), 110–11, 180–81.

⁷⁸ *Ibid.*, 183, 29.

⁷⁹ Locke, *Some Considerations on the Consequences of Lowering the Interest and Raising the Value of Money* (1691), in *The Works of John Locke*, ed. J. Law, 5 (London, 1801): 53, 60.

“working schools.” A full century before Bentham and his charity schools, Locke proposed that all children of the poor “shall be obliged to come” at the age of three to live in “schools” where the only subjects taught would be spinning and knitting. This would cure them of idleness, for they would then “from infancy be inured to work, which is of no small consequence to the making of them sober and industrious all their lives after.” Rounding up the children of the poor and incarcerating them in order to teach them industry and hard work would, Locke conceded, cost the parishes dearly; but it would ultimately prove profitable in the account books of the spinning and knitting managers, for “the earnings of the children [would] abat[e] the charge of their maintenance, and as much work [would] be required of each of them as they are reasonably able to perform,” so that “it will quickly pay its own charges with an overplus.”⁸⁰

From such Lockean schemes and the values they embody there is a direct link not only to Bentham but to the middle-class Protestant reformers of the late eighteenth century. Priestley’s proposal for *A Society for Encouraging the Industrious Poor*, submitted to Wilkinson the ironmaster in 1787, contains similar sentiments. But the clearest link is in the equally repressive plan offered by James Burgh to eliminate idleness and encourage industry. He proposed in his *Political Disquisitions* that the police and press gangs “seize all idle and disorderly persons, who have been three times complained of before a magistrate, and to set them to work during a certain time, for the benefit of great trading or manufacturing companies.”⁸¹ These are expressions neither of nostalgic country ideology nor of republicanism but of an ideology of hard work and industry. Middle-class reformers in the late eighteenth century were more likely to read the world and assess its institutions—economic, social, and political—in terms of the dialectic of industry and idleness than “virtue and commerce.” Here, too, they bore the indelible imprint of “that close reasoner, Mr. Locke.”

Chapter 5 of *The Second Treatise*, “On Property,” became the received wisdom in advanced radical circles in the late eighteenth century. Priestley described “a difference in industry” as introducing and legitimating inequalities of property, “so that in time some will become rich and others poor.” In turn, those with property are led to create civil society by “the desire of securing the undisturbed enjoyment of their possessions.”⁸² The philologist and radical Horne Tooke is a less likely spokesman for these views, but he, too, felt the impact of Locke’s economic ideas. His essay on the nature of property and wealth, published in 1795, begins by proclaiming that “Industry, the bodily labour of the human being is the foundation of all property.” The idle, however, “according to the original and natural constitution of things have no right or property in anything.” But, by his reckoning,

⁸⁰ Locke, *Some Thoughts Concerning Education*, 189–90. For Bentham, see Gertrude Himmelfarb, “Bentham’s Utopia: The National Charity Company,” *Journal of British Studies*, 10 (1970): 80–125.

⁸¹ James Burgh, *Political Disquisitions*, 3: 220–21. It should be noted that Francis Hutcheson, so favored by Gary Wills as the intellectual grey eminence behind Thomas Jefferson, went even further. Although Hutcheson opposed slavery of Africans, he insisted that slavery should be “the ordinary punishment of such idle vagrants, as, after proper admonitions and tryals of temporary servitude, cannot be engaged to support themselves and their families by any useful means”; Hutcheson, *A System of Moral Philosophy*, 2 (London, 1755): 202.

⁸² Priestley, *Lectures on History and General Policy*, 303, 297.

this was by no means the case in Tooke's England. With the exaggerated flair of the middle-class apologist, he asked, "Though it may be easily conceived that this was the case originally when men lived in a state of nature . . . , how is it that [today] the most industrious and laborious are scarcely able to procure even the common necessities of life" at the same time that "the idle, those who never work at all are rolling in luxury, and possess all the property in the kingdom, an inversion of the laws of nature"?⁸³

Middle-class reformers in the late eighteenth century used the older language of civic humanism and corruption, to be sure. They and their American friends, like Jefferson and Franklin, complained often of the corruption that hung heavy over Britain.⁸⁴ But corruption was a very different notion for these reformers from what it had been for the earlier Bolingbroke and his country ideology. A corrupt man for Burgh, Price, and Priestley was idle, profligate, unproductive, and lacking in talent and merit. A corrupt system was one in which such drones held important public offices, one in which privilege, not merit, distributed the prizes in the race of life, and one in which patronage insured the rule of unproductive—that is, corrupt—men of no ability instead of that of deserving men of talent. When Francis Place complained that "the whole system of our Government is essentially corrupt," he was not invoking a court-country equation with commerce and modernity, he was using a new public language that saw government as a reserve for privileged parasites. It was the same language used by the Sheffield reformer, Henry Yorke, in linking corruption to the aristocratic system:

From the privileged orders where birth supplies the lack of ability, virtue, knowledge and experience the executive officers of the nation are selected. . . . They preclude the industrious citizen from all honourable enterprise and patriotic exertion. . . . Intrigue and corruption are the only trades of the aristocracy. In these they excell. By these they enrich themselves. By such criminal means they monopolise all the offices of the church, the law and the army . . . ; they first corrupt you, and then they intrigue against you; they purchase you, in order to sell themselves. . . . The aristocracy desire to be distinguished from you, not to be distinguished by you. In a government where such a system is prevalent what is to be expected, but the extension of that corrupt influence by which it is upheld.⁸⁵

These useless idlers presided over a system that denied careers to the talented. The real nation was, as earlier in the century, seen as outside that corrupt government, but that nation was not a warmed-over Augustan country. Now it was the virtuous, hard-working, and frugal middle class and artisanry, who were as uninterested in a republican order of civic virtue as they were in an aristocratic order of deference and privilege. What they wanted was a meritocracy of talent. Had not Locke written, after all, that God "gave the world to the use of the Industrious and the Rational"?

⁸³ Tooke, *Causes and Effects of the National Debt and Paper Money on Real and Natural Property in the Present State of Civil Society* (London, 1795), 2–4.

⁸⁴ See Drew R. McCoy, "Benjamin Franklin's Vision of a Republican Political Economy for America," *William and Mary Quarterly*, 25 (1978): 605–28.

⁸⁵ Place, as quoted in Graham Wallas, *The Life of Francis Place, 1771–1854* (New York, 1919), 256; and Yorke, *Thoughts on Civil Government Addressed to the Disenfranchised Citizens of Sheffield* (London, 1794), 55–59.

These radical dissenters were the first fully developed theorists of the liberal ideal of equal opportunity. James Burgh, in attacking the Test and Corporation Acts, which denied religious dissenters important civil and military positions, demanded that England “give all an equal chance for rising to honours in the state according to merit.” Priestley described the reformers’ vision as one of “free access to honour and employments to every member of the state and free scope . . . given the exertion of every man’s ability.”⁸⁶ Anna L. Barbauld, a member of Priestley’s circle, made the same point in 1790. The natural equality of Lockean theory had by then become a natural right to equal opportunity. In opposition to aristocracy, these individualist liberals offered meritocracy. Barbauld did not ask for a favor but for “a natural and inalienable right,” which she claimed was hers and every dissenter’s.

To exclude us from jobs is no more reasonable than to exclude all those above five feet high or those whose birthdays are before the summer solstice. These are arbitrary and whimsical distinctions. . . . We want civil offices. And why should citizens not aspire to civil offices? Why should not the fair field of generous competition be freely opened to every one?⁸⁷

The issue was “power, place, and influence.”

Pocock and other revisionists have been quite right to see the court-commerce connection earlier in the century. But, by the late eighteenth century, the country reform tradition came to terms with the market and, indeed, in the hands of middle-class industrial dissent turned that reform tradition into a wholehearted ideology of the market. The court, while bound to the market and commerce from Walpole on, was enmeshed in the principle of patronage, which ultimately flew in the face of market notions of careers neutrally open to talent and hard work. It is here that the conflict emerges. Patronage and privilege are principles that pitted the court against the bourgeois reformers. Middle-class radicals inveighed against corrupt patronage, but it was a new sense of corruption, the corruption of jobs and places going to undeserving, untalented men of birth. It was the privileged court that in this period responded with a nostalgic defense of the ancient constitution, hierarchy, and paternalism. Its defenders ridiculed the leveling ideas of monied men and provincial bumpkins.

A court-country reading of the later eighteenth century becomes too confusing to be useful, because, with the emergence and eventual supremacy within the country “outs” of a class-conscious bourgeoisie, the court-commerce linkage becomes obsolete. In the eyes of the middle-class radical “outs,” the earlier equation was reversed. The “ins,” the court and all it stood for, were identified not with the market and commerce but with idle, unproductive privilege. The commercial and financial revolution stood behind the court-country split of the Augustan era. Its relevance receded, however, with the early years of the Industrial Revolution, when new dichotomous distinctions captured the fancy of reformers, not the least of which was virtuous commerce versus corrupt privilege. The marriage of

⁸⁶ Burgh, *Crito: or, Essays on Various Subjects* (London, 1767), 68; and Priestley, *Lectures on History and General Policy*, 320.

⁸⁷ Barbauld, *Address to Opposers of the Repeal of the Corporation and Test Acts* (London, 1790), 17–18.

industrial England with dissenter reform doomed court-country politics and introduced class politics.

What emerged in the course of the late eighteenth century—and most vividly in the writings of the middle-class radicals—was a new notion of virtue, one that dramatically rejects the assumptions of civic humanism. Citizenship and the public quest for the common good were replaced by economic productivity and hard work as the criteria of virtue. It is a mistake, however, to see this simply as a withdrawal from public activity to a private, self-centered realm. The transformation also involved a changed emphasis on the nature of public behavior. The moral and virtuous man was no longer defined by his civic activity but by his economic activity. One's duty was still to contribute to the public good, but this was best done through economic activity, which actually aimed at private gain. Self-centered economic productivity, not public citizenship, became the badge of the virtuous man.

A new cultural ideal was taking shape in the work and writings of these radicals. *Homo civicus* was being replaced by *homo oeconomicus*. In his letters, Josiah Wedgwood, Priestley's patron, described how he had "fallen in love" with and "made a mistress" of his pottery business. His productivity was merely to abide by the will of God. Wedgwood obeyed what he called the "eleventh commandment—Thou shalt not be idle." His friend, the dissenting cotton manufacturer Jedediah Strutt, was convinced, "whatever some Divines would teach to the contrary," that the "main business of the life of man was the getting of money."⁸⁸ The early classics of children's literature produced in England from 1760 to 1800 by these very same middle-class radicals in Priestley's circle contain few lectures or parables extolling civic responsibility (unlike *Émile*), but they continually praise productive hard work.⁸⁹ In Anna Barbauld's children's tale "True Heroism," she wrote that great men were no longer those who devote themselves to public life—"Kings, lords, generals, and prime ministers." There were new heroes, men who instead "invent useful arts, or discover important truths which may promote the comfort and happiness of unborn generations in the distant parts of the world. They act still an important part, and their claim to merit is generally more undoubted, than that of the former, because what they do is more certainly their own."⁹⁰ A pamphleteer of 1780 spelled out even more clearly who these new heroes were: "Consider the gradual steps of civilization from barbarism to refinement and you will not fail to discover that the progress of society from its lowest and worst to its highest and most perfect state, has been uniformly accompanied and chiefly promoted by the happy exertions of man in the character of a mechanic or engineer!"⁹¹

The middle-class radical praise of economic man no longer shares what Pocock

⁸⁸ [Wedgwood] *The Selected Letters of Josiah Wedgwood*, ed. A. Finer and G. Savage (London, 1965), 46, 247; and Strutt, as quoted in R. S. Fitton and A. P. Wadsworth, *The Strutts and the Arkwrights: A Study of the Early Factory System* (Manchester, 1972), 109–10.

⁸⁹ See my "Children's Literature and Bourgeois Ideology: Observations on Cultural and Industrial Capitalism in the Later Eighteenth Century," in Perez Zagorin, ed., *Culture and Politics: From Puritanism to the Enlightenment* (Berkeley and Los Angeles, 1980), 203–40.

⁹⁰ Barbauld, *Evenings at Home: or, The Juvenile Budget Opened*, 6 (Philadelphia, 1796): 223.

⁹¹ *Letters on the Utility and Policy of Employing Machines to Shorten Labour* (London, 1780), 3.

described as the republican dread of Aristotle's banausic men, who are "less than citizens because they specialised in the development of one's capacity."⁹² Specialization for the radicals, far from a sign of corruption, was a characteristic of virtuous man. It did not render economic man dependent on government or make him the servant of others, like priests, lawyers, rentiers, or soldiers. The specialist, like the entrepreneur, the scientist, the engineer, the inventor, or any man of talent, was the true social hero, who through his ingenious productivity and private pursuits shaped the public good. The older praise of the public citizen as a nonspecialized amateur smacked too much of the aristocratic rule of idle and untalented privilege. A self-conscious glorification of specialization against Aristotle's "ethos of *zōon politikon*" is implicit in an ideology that extols talent, merit, and skill. Hence, not just Adam Smith but a chorus of writers in the last decades of the eighteenth century sang the praises of specialization and the division of labor. The very heart of civic humanism was repudiated and its values reversed by the radical middle-class crusade to professionalize and specialize, to replace what it saw as corrupt political man with virtuous and productive economic man.

Josiah Wedgwood approached civic life as a specialist in industry and commerce. "Sunk again I find into politicks" was how he described himself, reluctantly having to leave his business for citizenship. Not "fame" but "money getting" was his concern. When his friend, the great engineer Brindley, died, Wedgwood noted that it was talents like his that truly benefited mankind. The public good done by such men of genius, the contribution to the commonweal by such men of "ingenuity and industry," far surpassed the contribution of political men, of "many noble lords." The economic benefactors "will be remembered with gratitude and respect" when the others "are totally forgotten." For Thomas Cooper, the industrialist and scientist who like Priestley eventually settled in America, virtue and privilege were incompatible. Only those with "insatiable ambition" could be able, wise, or virtuous.⁹³

THE MIDDLE CLASS WRAPPED ITSELF in this new notion of virtue. They were "not adorn'd, it's true with coats of arms and a long Parchment Pedigree of useless members of society, but deck'd with virtue and frugality." When Jedediah Strutt in composing his own epitaph wrote that "he had led a life of honesty and virtue," thoughts of country purity and citizenship could not have been further from his mind.⁹⁴ His life was virtuous compared to the corruption of the idle nobility and the wretched poor, for he had worked harder and contributed more with his talent, ingenuity, and industry to the increased productivity and wealth of his nation than they had. He was typical of a new species of self-centered virtuous men—men like

⁹² Pocock, *Politics, Language, and Time*, 92, and *The Machiavellian Moment*, 499.

⁹³ [Wedgwood] *Selected Letters*, 233, 81, 136, 182; and Cooper, *A Reply to Mr. Burke's Invective against Mr. Cooper and Mr. Watt* (London, 1792), 16.

⁹⁴ J. Stit, *A Sequel to the Friendly Advice to the Poor* (Manchester, 1756), 17; and Strutt, as quoted in Fitton and Wadsworth, *The Strutts and the Arkwrights*, 108.

those seen in Birmingham by an eighteenth-century chronicler of the middle class:

I was surprised at the place, but more so at the people: they were a species I had never seen: they possessed a vivacity I had never beheld: I had been among dreamers, but now I saw men awake: their very step along the street showed alacrity: Everyman seemed to know and prosecute his own affairs: the town was large, and full of inhabitants and those inhabitants full of industry.⁹⁵

When such self-centered, virtuous men addressed themselves to public issues, they did so less and less in terms of the paradigms and language of civic humanism or classical republicanism and more and more with the conceptual framework they knew best, the market. Joel Barlow—financial speculator, international entrepreneur, radical friend of Jefferson, Paine, Price, Wollstonecraft, Godwin, and Priestley—wrote of the French Revolution in his *Advice to the Privileged Orders in the Several States of Europe* in the language and the paradigms he knew best. He began, “It must be of vast importance to all classes of society . . . to calculate before hand what they are to gain or to lose by the approaching change; that like prudent stock jobbers, they may buy in or sell out, according as this great event shall effect them.”⁹⁶

Barlow and his friends, British and American, knew their Aristotle, their Machiavelli, and their Montesquieu. But they also knew their Locke. The world view of liberal individualism was fast pushing aside older paradigms during the last three decades of the century in the wake of the American crisis and the inventions of Watt and Arkwright. Two hundred years later, republican revisionism depicts these late eighteenth-century figures as preoccupied with public virtue and civic humanism and as uninterested in Lockean liberal ideals. When allowed to speak for themselves, however, these radicals seem to tell a different story.

⁹⁵ W. Hutton, *An History of Birmingham to the End of the Year 1780* (Birmingham, 1781), 63.

⁹⁶ Barlow, *Advice to the Privileged Orders in the Several States of Europe* (London, 1792), 3.

The Myth of Perry Miller

FRANCIS T. BUTTS

IN THIS AGE OF SOPHISTICATED SCHOLARSHIP in the field of American Puritanism, the work of Perry Miller would seem to need no introduction. Miller is acknowledged to be the premier American intellectual historian of the present century and the "giant" among students of colonial New England.¹ For three decades, beginning in the early 1930s, he taught American literature and history at Harvard. By the time of his death in 1963, his monumental studies were widely held to provide the standard interpretation of the development of New England culture. To this day his work continues to be cited as the point of departure for serious scholarship in the field.

Miller's stature has ensured that his portrait of colonial New England would become something of a commonplace. Historians have come to stereotype Miller, and a powerful consensus has formed concerning the content of his work. What he said about the dynamics of Puritan thought, or about New England's errand, the jeremiad, or declension, is confidently assumed to be a matter of common knowledge to Puritan scholars. A recent writer, for example, has remarked that "students of American intellectual history need no guide to the most commanding and justly famous work in our own scholarly tradition, Perry Miller's *The New England Mind*."² Indeed, some historians have felt that the familiarity of Miller's work has been detrimental to further scholarship. Not long ago another writer suggested that Miller's "standard definition" of the Puritan jeremiad "is too well known perhaps, for it seems to have fostered a series of misrepresentations both of the jeremiad and of the Puritan concept of errand."³ Whether historians agree or

This article represents an abbreviated statement of themes treated in my "Perry Miller and the Ordeal of American Freedom" (Ph.D. dissertation, Queen's University, 1980). I wish to thank Michele Dale, Klaus Hansen, Robert Malcolmson, George Rawlyk, Graham Reynolds, Joan Sherwood, and James Stayer for their advice and assistance.

¹ Darrett B. Rutman, *American Puritanism: Faith and Practice* (Philadelphia, 1970), 130. For representative examples of the praise that has been bestowed upon Miller, see George Langdon, Review of Miller, *Nature's Nation* (1967), in the *New England Quarterly*, 41 (1968): 148; Edmund S. Morgan, "The Historians of Early New England," in Ray Allen Billington, ed., *The Reinterpretation of Early American History* (San Marino, Calif., 1966), 52; and Richard Schlatter, "The Puritan Strain," in John Higham, ed., *The Reconstruction of American History* (New York, 1962), 35.

² David A. Hollinger, "Historians and the Discourse of Intellectuals," in John Higham and Paul Conkin, eds., *New Directions in American Intellectual History* (Baltimore, 1979), 46-47.

³ Sacvan Bercovitch, *The American Jeremiad* (Madison, Wis., 1978), 4, 5.

disagree with Miller, however, they have not been inclined to wrangle over the substance of his history.

Since Miller's death, his interpretive framework has come under militant assault. Toward the end of his career, scholars were becoming restive under it. There was a growing feeling among specialists that creative work in American Puritan studies demanded liberation from the stultifying "shadow" of Miller's "intellectual edifice."⁴ A reaction against him set in, which soon mushroomed into a major scholarly industry, one that has helped make the reputations of some of today's leading Puritan scholars. This reaction has been particularly strong among the new social historians of the 1960s—not terribly surprising given Miller's apparent contempt for their enterprise. For the most part, Miller's opponents have subscribed in varying degrees to the commonplace view of him. They may still genuflect before his accomplishments, but they treat him primarily as a foil.

But the figure of Perry Miller, as historians now ordinarily understand him, has more the character of a myth than the substance of reality. To an appreciable extent, discussion of him has dwelled upon a misleading caricature. Thus, the confidence historians have placed in their comprehension of Miller's "standard definitions" of Puritan history is unwarranted. And the consequence is that, for the most part, Miller's critics have been tilting at a straw man. This state of affairs is especially unfortunate because it has obscured the deeper meaning of the work of one of the most eminent practitioners of American history. That veil needs to be lifted, that stereotype needs to be exposed, and that caricature needs to be redrawn into a more accurate portrait of the man and his work.

IT HAS BECOME THE CONVENTIONAL WISDOM to think of Perry Miller as a narrow historian of ideas, a man who was almost exclusively interested in formal thought. The prevailing view understands Miller to have been essentially a rationalist and a philosophical idealist. Because of his alleged concentration on abstract thought, he has frequently been charged with having given short shrift to the passionate side of religious life. Although Miller supposedly ascribed a duality to the mind, based on the interaction of piety and intellect, he has been accused of emphasizing the head to the detriment of the heart. Alan Simpson expressed this criticism in 1956, when he taxed Miller for having "told us too much about the Puritan mind and not enough about the Puritan's feelings." A more extreme instance of this sort of critique appears in Sacvan Bercovitch's assertion that "Perry Miller has described the New England Mind as a kind of logic-machine run by the principles of the Ramist dialect and oiled by rigid pedantry." In 1970, Michael McGiffert discerned among Puritan scholars a general belief that "Miller overstated the rigorous intellectuality of Puritanism, perhaps because . . . he was working 'with a rationalistic-idealistic model of the human mind.'"⁵

⁴ John C. Crowell, "Perry Miller and Typology," *Andover Newton Quarterly*, 17 (1977): 227. Also see Darrett B. Rutman, Review of David D. Hall, *The Faithful Shepherd: A History of the New England Ministry in the Seventeenth Century* (1972), in the *New England Quarterly*, 46 (1973): 310.

⁵ Simpson, *Puritanism in Old and New England* (Chicago, 1955), 21; Bercovitch, "Typology in Puritan New England: The Williams-Cotton Controversy Reassessed," *American Quarterly*, 19 (1967): 189; and McGiffert, "American Puritan Studies in the 1960's," *William and Mary Quarterly*, 3d ser., 27 (1970): 52.

Since Miller purportedly focused on the “inner” life of abstract ideas, he has commonly been reproved for ignoring, indeed disparaging, social and economic history. A fundamental premise of his work, it is said, is the principle of the autonomy of thought. Miller’s studies have thus been seen as portraits of disembodied minds. In the orthodox view, he approached the intellect as if it were a brain in a bottle. The subjective, rather than the objective, factors of history have been taken to be his chief concerns. This assessment has been most forcefully expressed by Robert A. Skotheim in *American Intellectual Histories and Historians* (1966). Skotheim has repeatedly pointed to “Miller’s interest in thought for its own sake,” which led Miller to use what Skotheim labeled a “non-environmental technique of describing ideas.” Therefore, *Orthodoxy in Massachusetts* (1933), Miller’s first book, “was written without regard for the social and economic conditions which might have affected Puritan thought.” According to Skotheim, the epitome of Miller’s method can be found in the first volume of *The New England Mind* (1939), which “was an exceedingly detailed study of the content and structure of ideas, with almost no attention given to the environment from which these ideas came. In that book, Miller rarely mentioned “social and economic background information as being relevant in any way to a fuller understanding of Puritan thought.”⁶

Miller’s reputation for abstracted intellectualism has become an enduring feature of critical discussion. In a recent instance, Robert Darnton has coupled Miller with Arthur O. Lovejoy and asserted that both men “raised the level of intellectual history by stripping it of any concern for social context.” Thus, Miller encouraged Americanists “to chase after abstractions—myths, symbols, and images”—so that “by the 1960s the American Studies movement had cut American intellectual history free of its moorings in social history and had drifted off in pursuit of a disembodied national mind.”⁷

Miller’s apparent belief in the autonomy of thought has led commentators to portray him as something of a Platonist, a scholar who in principle regarded ideas as essences independent of space and time. In Robert Middlekauff’s estimation, Miller considered certain ideas to be forms, and “form for Perry Miller may be thought of as having existence outside of experience; form indeed may be outside of history.” Therefore, Miller wrote as though “ideas take on a life of their own.” Along with Skotheim, Middlekauff perceived that Miller did not hold a pragmatic or instrumental view of thought. Ideas “are not simply constructs devised by men trying to cope with the world or to penetrate its mysteries.” Instead, “they seem almost to have some inner force impelling men to take up unanticipated positions.” Miller’s quasi Platonism allegedly disposed him to describe the New England mind in nonevolutionary terms. Again and again critics have remarked, to use Middlekauff’s words, that Miller “not only ignores experience” but also “deals with the Puritan mind as if it were static.”⁸ In summary, the orthodox view of Miller’s

⁶ Skotheim, *American Intellectual Histories and Historians* (Princeton, 1966), 191, 192, 197, 196. As evidence for Miller’s narrow vision of Puritanism, Skotheim cited Miller’s notorious declaration that he intended in *Orthodoxy in Massachusetts* “to tell of a great folk movement with an utter disregard of the economic and social factors”; Miller, *Orthodoxy in Massachusetts* (1933; reprint edn., New York, 1970), xxv.

⁷ Darnton, “Intellectual and Cultural History,” in Michael Kammen, ed., *The Past before Us* (Ithaca, N.Y., 1980), 329.

⁸ Middlekauff, “Perry Miller,” in Marcus Cunliffe and Robin W. Winks, eds., *Pastmasters: Some Essays on*

working assumptions about the mind and intellectual history holds that he asserted the independent status of rational thought, that he slighted religious emotion, that he lacked interest in matters external to the mind, and that he dealt with ideas as if they are rigid, unchanging entities, transcending the ebb and flow of events.

This stereotype has deeply colored the reading of Miller's historical themes. In connection with his supposed static handling of ideas, Miller has often been characterized as giving to the New England mind a uniform, enduring, monolithic shape, which enabled him to ignore the clutter of differences relating to both time and persons. Coherence, rather than diversity, is thus the keynote of his history of Puritan thought. As evidence, critics have cited Miller's bold assertion that, since "the first three generations in New England paid almost unbroken allegiance to a uniform body of thought," he could take "the liberty of treating the whole literature as though it were the product of a single intelligence." Miller went on to say that, "in most instances, it is a matter of complete indifference or chance that a quotation comes from Cotton instead of Hooker," because "all writers were in substantial agreement" on the main tenets of belief.⁹ So pervasive has the stereotype of Miller's Puritan monolith become that Michael McGiffert thought it "probably idle to demur that Miller's treatment of the Puritans belied, to a fair degree, his 'monolithic' titles."¹⁰

The image of monolithic Puritanism has influenced interpretation of Miller's notion of the Puritan mission. Since Miller supposedly presented the founders of New England as self-conscious, deliberate, and confident men, who knew what they wanted, the Puritan errand becomes a tale of the unswerving pursuit of highly articulate goals. In these terms, New World circumstances had little to do with the establishment of orthodoxy in Massachusetts. The Puritans came to New England with a precise blueprint of their ideal church and civil order, and they proceeded in a straightforward manner to erect it. The Puritan errand was defined before the settlement of Massachusetts, carried there by the first generation.

According to this reading of Miller, the high point of the Puritans' enterprise, with respect both to the fervor of their piety and to the realization of their longed-for "spiritual Utopia," was reached during the early years of the Bay Colony. Thereafter, Miller's tale becomes the "melancholy story" of declension. The great edifice of the first generation slowly crumbled from both within and without. Puritan religiosity waned as society grew increasingly secular. Uniformity gave way

American Historians (New York, 1969), 174, 182. Robert Skotheim suggested that "Miller interpreted ideas less pragmatically than any other American historian of ideas"; *American Intellectual Histories and Historians*, 191. For other instances of the conventional evaluation of Miller, see Bernard Bailyn, Review of Miller, *The New England Mind: From Colony to Province* (1953), in the *New England Quarterly*, 27 (1954): 112-18; David Hackett Fischer, *Historians' Fallacies: Toward a Logic of Historical Thought* (New York, 1970), 195-99; James Fulton Maclear, "The Heart of New England Rent: The Mystical Element in Early Puritan History," *Mississippi Valley Historical Review*, 42 (1955-56): 621-22; Morgan, "Historians of Early New England," 52-53; Rutman, *American Puritanism*, x-xi; Schlatter, "The Puritan Strain," 36-38; and George Selement, "Perry Miller: A Note on His Sources in *The New England Mind: The Seventeenth Century*," *William and Mary Quarterly*, 3d ser., 31 (1974): 453-64.

⁹ Miller, *The New England Mind: The Seventeenth Century* (1939; reprint edn., Boston, 1961), vii, ix.

¹⁰ McGiffert, "American Puritan Studies," 42. McGiffert's article gives a good sampling of revisionist positions, particularly among the work of social historians, who have vigorously asserted the "pluralistic" origins of New England in opposition to what they have fancied is Miller's monolithic approach.

to diversity; self-assurance to confusion. The second and third generations were lesser men than the founders. Miller presumably admired the founders because they had achieved a precarious balance between piety and intellect—one that, lamentably, could not last—and therefore made the tension between reason and the spirit the central impulse behind intellectual change. Reason persistently encroached upon faith and gradually smothered it, leaving behind the shell of formal doctrine.¹¹ As faith expired and the vision of the holy commonwealth dimmed, as ordinary men became absorbed in secular pursuits and factionalism set in, the ministers, guardians of tradition, came to bewail the turn of events. Their obsessive jeremiads were the pessimistic cries of a people whose way had been lost. Wedded to the past, yet propelled into the future, the New England mind, as typified by Cotton Mather, became weak, twisted, and neurotic. Eventually, in a desperate move to recapture the past, Mather formulated a “naïve pietism,” which was in reality a “travesty” of the faith of the first generation. In his attempts to resurrect the authority of the clergy, Mather vulgarized religious faith and thereby further promoted the secularization of the New England spirit. According to this interpretation, Miller “missed no opportunity to show his distaste for Mather.”¹² From his apparent invidious contrast between Mather’s generation and the founders, Miller’s sympathies had to lie with the latter. In other words, he mourned the passing of the Bible Commonwealth.

Such are the components of the Miller stereotype. Although it is most prominent in the thinking of social historians, historiographers and intellectual historians share it as well. The commonplace view of Miller has not, however, entirely dominated critical discussion. A small, intermittent, but important, divergent strain has begun to surface, as commentators are gradually coming to appreciate the passion behind Miller’s writing, his fascination with the emotive aspects of life, his preoccupation with the problems of the American identity, and his ultimate involvement with what Alan Heimert has called “metahistorical” questions.¹³ Especially in the work of Gene Wise, moreover, there is the beginning of an understanding of Miller’s instrumental conception of the meaning of ideas. Unfortunately, the insights of this literature tend to be scattered, impressionistic, and undeveloped. They do not amount to a definitive refutation of the conventional wisdom. Too often those who are groping toward a new understanding of Miller remain tied to basic components of the orthodox view. This is notably the case with

¹¹ Robert Middlekauff, “Piety and Intellect in Puritanism,” *William and Mary Quarterly*, 3d ser., 22 (1965): 457–58, and “Perry Miller,” 173–77. The term “spiritual Utopia” is Miller’s own; *Orthodoxy in Massachusetts*, 212.

¹² Bercovitch, *The American Jeremiad*, 4–10; and Edmund S. Morgan, Review of Miller, *The New England Mind: From Colony to Province* (1953), in the *William and Mary Quarterly*, 3d ser., 11 (1954): 295, and “The Puritan You Love to Hate,” a review of David Levin, *Cotton Mather, The Young Life of the Lord’s Remembrancer, 1663–1703* (1978), in the *New York Review of Books*, January 25, 1979, p. 31.

¹³ The most significant examples of this literature are Heimert, “Perry Miller: An Appreciation,” *Harvard Review*, 2 (1964): 30–48; David A. Hollinger, “Perry Miller and Philosophical History,” *History and Theory*, 7 (1968): 189–202; Karen Lystra, “Perry Miller and American Puritan Studies: A Case Study in Scholarly Community” (Ph.D. dissertation, Case Western Reserve University, 1973); Richard Reinitz, “Perry Miller and Recent American Historiography,” *Bulletin of the British Association for American Studies*, no. 8 (1964): 27–35; and Gene Wise, *American Historical Explanations: A Strategy for Grounded Inquiry* (Homewood, Ill., 1973), and “Implicit Irony in Perry Miller’s *New England Mind*,” *Journal of the History of Ideas*, 29 (1968): 579–600.

David A. Hollinger's stimulating essay, "Perry Miller and Philosophical History," which mistakenly argues that Miller's world view was framed according to the classic subject-object and head-heart dualisms of modern philosophy.¹⁴ It is not the purpose of the present essay to examine in detail these initiatives toward a reinterpretation of Miller; the power and durability of the orthodox view demand that it receive primary attention.¹⁵

THE WEAKNESSES OF THE MILLER STEREOTYPE can be easily demonstrated. Far from approaching Puritanism primarily as a body of formal doctrine, Miller was emphatic in defining it in terms of a particular mood—what he called "the Augustinian strain of piety." At the very outset of his study of the New England mind, he explicitly stated that "in the history of thought what is spoken is less important than what is felt." Thus, to understand the dynamics of Puritanism, "we must take account not so much of systematic theology as of piety." As he observed elsewhere, "though the Puritan did indeed live much of his life by logic, he did not so live all of it or even the most important part of it. Dialectics and syllogism do not account for the driving force of the Massachusetts settlers."¹⁶ Contrary to the stereotype, Miller anchored his account of the mentality of Puritan New England in its particular emotional temperament.

The heart of the Augustinian mood, as Miller depicted it, was a profound sense of man's smallness in a universe that was both mysterious and majestic, frightening and elating. Puritans acutely felt the abyss that separated them from what they called God. Though an atheist, Miller was exceptionally sensitive to the existential themes in Christianity. Alienation, anguish, doubt, despair, and yearning for reconciliation are the keynotes of his portrait of the Augustinian sensibility. Miller regarded the Christian Gospels as "a great construct which man himself has devised for symbolizing the riddle of his existence." The same approach informs his Puritan studies. His Puritans responded to what he took to be man's universal predicament. Puritanism was "one more instance of a recurrent spiritual answer to interrogations eternally posed by human existence." Its "impetus came from an urgent sense of man's predicament, from a mood so deep that it could never be completely articulated." The dominant feeling of the Puritan temperament was that "man is not at home within the universe." The core of its piety "was its sense of the overwhelming anguish to which man is always subject." Anguish arose "from the realization that the natural man, standing alone in the universe, is not only minute and insignificant, but completely out of touch with both justice and beauty." From an empty soul, afflicted with anguish, there welled up a "consuming desire for fullness" and an "insatiable quest" for an ecstatic knowledge of, and reunion with,

¹⁴ Hollinger, "Perry Miller and Philosophical History," 189–91.

¹⁵ I acknowledge my debt to this reinterpreted literature; for a fuller analysis of these divergent strains, see my "Perry Miller and the Ordeal of American Freedom," 43–54.

¹⁶ Miller, *The New England Mind: The Seventeenth Century*, 14, and Miller and Thomas H. Johnson, eds., *The Puritans*, 1 (1938; reprint edn., New York, 1963), 282.

God.¹⁷ Miller stressed the agony and the ecstasy that surged within Puritanism in no uncertain terms.

Here lies the basis of Miller's understanding of Puritan psychology. In "the irrepressible demand of the soul for this knowledge" he located "the driving force of the piety," and thus also of the power, behind Puritan thought.¹⁸ The dynamics of the Puritan mind were not initially a matter of tension between piety and intellect. The tension originated within man's emotional experience. It arose out of man's sense of loss and his restless, passionate thirst for wholeness—that is, out of his existential experience. The Augustinian "strain" to which Miller referred may be a play on words, identifying the emotional tension within the mood as much as the mood's pedigree. Because the strain within the piety preoccupied man prior to logic or theology, the "real being" of Puritanism "was not in its doctrines but behind them." The articulation of religious doctrines was but the attempt to give formal expression to what Miller thought was man's tormenting primitive sentiment. As he affirmed, "Puritan theology was an effort to externalize and systematize this subjective mood." For him ideas had no independent powers of their own. They were not mysteriously operating, quasi-Platonic forms. The conflicting emotions within the Puritan piety formed "the temperamental bias behind the thought." Miller could not have been more definite about the existential source of Puritan theology than when he stated that "the doctrines of original sin, of the depravity of man, and of irresistible grace were not embraced for their logic, but out of a hunger of the human spirit and an anxiety of the soul."¹⁹ The tensions or paradoxes within Puritan thought did not at first arise because head was set against heart. Rather, paradoxes within the theology reflected the more basic paradoxes of the soul—of man's conflicting sense of both despair and exaltation.

In his approach to the Puritan mind, Miller interpreted spiritual torment according to an instrumental view of thought. Miller adhered to what he took to be a basic proposition of pragmatism, one that he found expressed by Charles Sanders Peirce. Of Peirce's essay "The Fixation of Belief" Miller wrote,

Peirce's point, which is the starting point for all his thinking, and in a sense for all varieties of Pragmatism, is that thinking of any kind, logical, scientific, religious, does not come naturally to man but is forced upon him. If man could remain comfortable with a ready-made set of opinions, he would never bestir himself to ask questions; but life is puzzling and disturbing, and causes even the least intellectual to doubt. Now doubt is uncomfortable, and profound doubt is agony. The organism tries to escape, and belief is to be defined as whatever will administer relief.²⁰

The function of thought is to allay anxiety. Philosophical ideas and theological doctrines have power because, in providing an explanation for man's unhappy lot, they give him a measure of assurance and comfort. This view is evident in Miller's

¹⁷ Miller, Review of Reinhold Niebuhr, *Faith and History* (1949), in *Nation*, August 6, 1949, pp. 138–39, and *The New England Mind: The Seventeenth Century*, 4, 7, 8, 22, 3.

¹⁸ Miller, *The New England Mind: The Seventeenth Century*, 8.

¹⁹ *Ibid.*, 4, 5, 6, 22.

²⁰ Miller, ed., *American Thought: Civil War to World War I* (New York, 1954), xxxi–xxxii.

remarks on the value of Ramist logic and covenant theology. Ramist logic was attractive precisely for the emotional security it offered. By its means "the laws of God found in the Bible were hypostatized . . . into never-failing realities, as endurable as facts, and from that assurance Puritanism got its strength and confidence." Likewise, Miller found that "one of the principal aims of the [covenant] doctrine had obviously been to provide safe and certain grounds for assurance; when it was invented men were overwhelmed by the sense of a transcendent and omnipotent divinity, and it erected a platform on which the saint might stand secure, though it had to build that refuge upon the pledged word of God."²¹ Miller's explanation of the Puritan mind thus fused a deep sense of existential anguish with a pragmatic understanding of the purpose of thinking.

Miller's long analysis of Puritan scholasticism must be comprehended in this light. Puritan cerebration was prompted by emotional need, and Puritan zeal for logic was testimony not to some abstract intellectual interest but to the turmoil of the soul. Anxiety as a spur to intellectual activity is a common theme in Miller's writing, exemplified, for example, in his comments on the sermons of Thomas Hooker.²² Miller observed that "anyone who ever saw himself as pitilessly as Hooker requires would thereafter spend his days and nights in feverish exertions to lift himself out of such a mire of depravity." Hooker's analyses of what he understood to be man's struggle with evil "uncover the sources of that most titanic energy of which the Puritans were so abundantly possessed." Miller got at the same point elsewhere: "the primitive order of New England was an ingenious method for institutionalizing . . . [the] 'remorse of conscience,' by which he meant "the gnawing bite of that wit which festers within a man and which he cannot control."²³ Relentless self-examination was a consequence, and theology proceeded from it. The immense edifice of Puritan theology was thus erected in the hope of overcoming the deeply disquieting sense of disproportion between man and God.

In fact, so nagging was the craving of the Puritan thinkers for spiritual quietude that, "at whatever cost to consistency," they were determined to bring "God to time and reason."²⁴ The Puritans remained faithful to the doctrine of predestination in part because its guarantee of salvation could be consoling. Yet, they also sought the comfort given by a rational explanation of the workings of God's secret will. "Intellectual legerdemain" was the only way in which the implicit contradiction between a belief in God's mysterious, absolute will and a belief in his rational

²¹ Miller, *The New England Mind: The Seventeenth Century*, 148, and "Solomon Stoddard, 1643–1729," *Harvard Theological Review*, 34 (1941): 286.

²² In Miller's writing, the classic instance of the connection between anxiety and effort appears in his treatment of Cotton Mather. Of the moral guardians of New England, such as Mather, Miller wrote that they "were cudgeling their brains, trying to keep up with change; on the surface the decades between 1700 and 1730 seem almost placid, but underneath was a torrent of anxiety"; Miller, *The New England Mind: From Colony to Province* (1953; reprint edn., Boston, 1961), 375. This theme is also central to Miller's interpretation of Theodore Parker. Miller attributed Parker's "maniacal ransacking of the world's scholarship" to "neurotic compulsions" arising from the inherent instability of liberalism. Parker's philosophy permitted him "no surcease from insecurity and anxiety." He "dramatized the hitherto concealed terror within the assurance of progress." Miller, *Nature's Nation* (Cambridge, Mass., 1967), 148–49.

²³ Miller, *The Puritans*, 283, and *The Responsibility of Mind in a Civilization of Machines* (Amherst, Mass., 1979), 177.

²⁴ Miller, *Errand into the Wilderness* (1956; reprint edn., New York, 1964), 54, 56. Also see *ibid.*, 93–97.

dealing with men could be resolved. As portrayed by Miller, the Puritan divines, far from being rigorous logicians, were master sophists who wanted to “have their God without the chaos, the God of Calvin and yet a world that could be known and understood.”²⁵

To Miller it was axiomatic that the Puritan intellectuals were engaged in what even they fundamentally knew to be a specious enterprise—one that he implied was analogous to that of K, the surveyor, in Franz Kafka’s *The Castle*.²⁶ Absolute certainty is a delusion. Finite man will never be able to make the ways of God, or of the universe, entirely transparent to human understanding. Miller pointed out the inevitable futility of Puritan rationalism when, referring to “the Sysiphean [*sic*] labors of the divines,” he described Puritans as “again and again almost finishing exhaustive systematizations of the doctrine, only to have their structure collapse because some obstinate fact simply will not fit in.” The consequence, however, of all their cogitation was a gradual evolution in thought. Repeated frustration of their attempts to bridge the abyss between the sense of self and that of God gave rise to intellectual change. As Miller put it, “the space between revelation and the inconceivable absolute . . . was the portal through which ran the highway of intellectual development.”²⁷ Thus, in Miller’s account the unsettling feeling of human weakness, heightened by the Augustinian influence, was the ultimate motor of intellectual change. Far from presenting a static picture of an abstract body of thought, the first volume of *The New England Mind* is dramatic, developmental, and ironic. It sets a spiritual problem and shows how, from the very passionate quest for psychological stability, intellectual change unexpectedly issued. Already in his early article, “The Marrow of Puritan Divinity,” Miller unmistakably displayed his developmental conception of the history of Puritan thought: “pristine doctrine was not rigorous, ironclad, and inflexible; it had in it the elements of complexity, the seeds of future growth, making for diversity and contradiction.”²⁸

Without question, a substantial portion of the first volume of *The New England Mind* is devoted to illuminating points of conflict between theological and philosophical ideas. Miller’s Puritans espoused a host of mutually contradictory ideas, derived from a variety of religious and secular sources. And Miller sometimes talked loosely about the two halves of the Puritan mind, piety and intellect, and the resultant tension.²⁹ His own lack of clarity is, therefore, partially responsible for the mistaken understanding of his psychology of Puritanism. Nevertheless, such remarks do not accurately reflect his deepest understanding of the Puritan psyche. When he referred to the tension between piety and intellect, or faith and reason, he was simply using a shorthand to distinguish theological from philosophical doctrines. But he did not mean to imply that such tension on the abstract level was the mainspring of the Puritan mind. Careful examination of his use of the word “piety”

²⁵ Miller, *The New England Mind: The Seventeenth Century*, 176, 158. Also see *ibid.*, 286–87, 385, 394–95, 449. The same theme can also be found in Miller, “The Puritan Theory of the Sacraments in Seventeenth-Century New England,” *Catholic Historical Review*, 22 (1937): 421–22.

²⁶ Miller, *Errand into the Wilderness*, 162, and Jonathan Edwards (1949; reprint edn., New York, 1967), 100.

²⁷ Miller, *The New England Mind: The Seventeenth Century*, 26, 21. Also see *ibid.*, 399.

²⁸ Miller, *Errand into the Wilderness*, 97–98.

²⁹ Miller, *The New England Mind: The Seventeenth Century*, 69, 71, 76–77, 112.

shows that he employed it to refer to different levels of experience. In an outward sense it referred to the doctrines of Reformed theology, which were formal expressions of a more fundamental mood that comprised the heart of piety. The first chapters of *The New England Mind* were an attempt to grasp "the piety . . . on the emotional and non-theological level." Miller quite explicitly stated his purpose in these chapters: he was "seeking to delineate the inner core of Puritan sensibility apart from the dialectic and the doctrine," even though he realized that "in Puritan life the two were never so separated," were "indeed inseparable." His method of analysis was dictated by his belief that "as long as [Puritanism] remained alive, its real being was not in its doctrines but behind them."³⁰ This mood, this anguish over man's predicament, this yearning for reconciliation with God, was the motive force behind both Puritan philosophy and Puritan theology. They were alternative strategies designed to ease the Puritans' spiritual distress. Unfortunately for the Puritans, the formal doctrines of faith and reason do not easily mix, or can be made to mix only by a kind of intellectual sleight of hand. Miller understood the clash of abstract ideas within Puritan thought to be an expression of the more basic ambivalences and paradoxes of the human soul—of man's simultaneous sense of weakness and strength, passivity and activity, fear and courage, and of his thirst for transcendence and wish for oblivion.

The poles of the Augustinian piety were either despair or transcendent elation. These feelings could easily be translated into the socially destructive behavior of fatalist inertia or Antinomian disregard for moral law. The Augustinian piety was unstable and, hence, potentially dangerous to social order. Central to Miller's account of the Puritan mentality was the belief that covenant theology was developed to provide the basis for moral obligation as well as for individual assurance of salvation. In routinizing the relationship between man and God, covenant theology promised to stabilize personal behavior, which would in turn foster social cohesion. Miller never forgot that theological propositions can have vital social and political ramifications. Covenant theory was important because upon it was built the theoretical justification of the Puritan state:

This conception was of tremendous value to the leaders of Massachusetts, not only in the realm of faith and personal conduct, but just as much in the realm of politics and society. The sphere of moral conduct includes more than such matters as individual honesty or

³⁰ *Ibid.*, 59, 6, 4. Recently, Stanford J. Searl, Jr., has drawn attention to Miller's belief "that behind and beneath the elaborate system of theology lies an underlying, inarticulate feeling." Unfortunately, Searl has not drawn the proper conclusions from his insight, tending instead to lapse into the dualisms of the commonplace view. He concluded that, "most of the time, Miller writes about the tension or conflict between piety and intellect." Perry Miller as Artist: Piety and Imagination in *The New England Mind: The Seventeenth Century*, *Early American Literature*, 12 (1977-78): 221, 230. Miller's belief that the heart of piety resided in an emotive core with a logic all its own is clearly exemplified in his remarks on the power of the doctrine of predestination. Understood rationally, predestination seems to encourage passivity and fatalism. Yet, Miller was exasperated with the superficiality of those who were content to dismiss the doctrine by pointing out this consequence. He maintained that for the Puritans "the doctrine of predestination did not have as a psychological consequence the surrender of all volition"; instead, "it was a powerful stimulus to activity." Miller made this same point in evaluating John Bunyan's *Pilgrim's Progress*: "What in theology is a logical riddle of fate and freedom becomes in the realm of art a resolution of assurance and effort." Indeed, "only when the theology of 'predestination' is seen in these less technical terms," Miller insisted, does "its vitality as a living faith and its strength as a sustaining philosophy become comprehensible." Miller, *Responsibility of Mind*, 136, 69, and *The Puritans*, 57. Also see Miller, *Jonathan Edwards*, 328-30.

chastity; it includes participation in the corporate organization and the regulation of men in the body politic. The covenant theology becomes, therefore, the theoretical foundation both for metaphysics and for the state and the church in New England.³¹

Frequently Miller called attention to the “pragmatic” purposes of covenant theory. In the very book that has been deemed to be the epitome of his “non-environmental” approach to thought, he concluded that,

as we have found repeatedly suggested by the tenor of this argument, the doctrine must bear some intimate relation to contemporaneous social history, to the points of view which Puritans were defending in the political and economic struggles of the century, to the alliance of Puritanism and the commonlaw. Perhaps it would not be amiss to say that though the covenant doctrine was elaborated by orthodox Puritans against Arminianism and Antinomianism, yet in their attempt to fend off these heresies the orthodox took up many ideas not so much for theological as for social and economic reasons.

As an intellectual historian, Miller’s ultimate interest was not in “thought for its own sake.” Just the opposite is true. He suggested that, “when the federal theologians were viewed historically, they seem to have served not so much the cause of their creed as of their party, and since their party dominated the scene in New England, their theology becomes of most lasting importance as an aspect of the political and social order.”³² From his first book, *Orthodoxy in Massachusetts*, Miller treated ideas as having value to the Puritans precisely for their social utility. His history therefore encompasses a concern for both the personal and the social aspects of religion.

Given Miller’s emphasis on the social utility of ideas, his professed “utter disregard of the economic and social factors” in the founding of Massachusetts does not really amount to a blanket condemnation of social history. It reflects more the intemperate vehemence of youth. Closer reading shows that the real focus of Miller’s criticism is the determinism that he saw implicit in the explanations of social historians. As a maturer man he was more careful to explain that he considered himself “the last to decry” the scholarship of social historians. He felt their work was indispensable to an understanding of history, yet he insisted, as he always had, that their interpretive framework was “not getting at the fundamental themes” of American history.³³

³¹ Miller, *Errand into the Wilderness*, 89.

³² Miller, *The New England Mind: The Seventeenth Century*, 397. In his review of this work, Herbert W. Schneider recognized Miller’s stress on the political and social functions of Puritan doctrine. Schneider did complain that Miller might have explained this connection more directly; “fortunately the volume contains so many scattered references to what Puritans did ‘that the discerning reader readily gets a sense of the practical meanings embedded in the theological arguments. This is particularly true of the last chapters on the Covenants of Grace, Society, and Church, in which the social implications of the Congregational theory of grace become evident.’” Schneider, Review of Miller, *The New England Mind: The Seventeenth Century* (1939), in the *Journal of the History of Ideas*, 1 (1940): 120. More recently, Felix Gilbert has observed, “The problem with which Perry Miller deals in his *New England Mind* [*The Seventeenth Century*] is similar to that which forms the subject of Troeltsch’s *Social Teachings of the Christian Churches*: The influence of religious beliefs and attitudes on social life and social action. . . . Perry Miller demonstrated that a change in one area of thought involves a realignment of thought and action in all other spheres. The assumption on which Miller’s “intellectual history” is based and which distinguishes his treatment from previous ones is that of the interconnected character of man’s concept of life.” Gilbert, “Intellectual History: Its Aims and Methods,” *Daedalus*, 100 (1971): 82.

³³ Miller, *Errand into the Wilderness*, vii–viii.

IF THE NOTION THAT PERRY MILLER was a student of narrow, abstract thought is false, then the standard interpretation of his scheme of New England history, which presupposes that notion, collapses. A close look at the cases made by some prominent revisionists serves to illustrate how the Miller caricature has warped the interpretation of his writing, how the critics' reading of Miller is clearly influenced by the stereotype of his overweening preoccupation with abstract ideas. These examples also demonstrate that the revisionists have rarely broken new interpretive ground, however much they have contributed to our understanding of Puritan thought and life. Their assessments of Miller are mistaken. Hence, their work does not amount to a revision but, instead, remains well within Miller's general interpretive framework on those matters ostensibly in dispute.

In a number of books and articles David D. Hall has advertised his dissent from Miller. Hall is an outspoken "pluralist" who declared that New England Congregationalism must be understood "as a fluid, evolving polity." He has judged Miller's history of the founding of Massachusetts to be rigid, timeless, and divorced from social reality—that is, he has accepted the basic features of the standard criticism of Miller's excessive intellectualism. Miller "minimized the restlessness within Puritanism."³⁴ This stance disposed Miller to attribute to Puritanism a static coherence that makes little allowance for its dynamic impulses or for its evolving character under shifting social circumstances. Because of his ideational fixation, Miller's account "exaggerates the value of denominational categories." He ignored the inchoate condition of Puritan ecclesiastical theory prior to the Great Migration and thus presented nonseparating Congregationalism as a "discrete system" of church organization, worked out in detail by theoreticians in Old England and transplanted to Massachusetts intact, without significant alteration. The New England Way represented the simple "unfolding of previously developed plans." Hall mentioned that occasionally Miller "treated congregationalism as an evolving system," but he explained that Miller regarded those innovations that did arise in New England as no more than "steps toward the achievement of orthodoxy—the closing of loopholes, so to speak, in an otherwise perfect system." Because Miller made the ideal of religious "uniformity the main goal of Ames and the preachers in New England, he did not recognize the dynamic interplay of values in their system."³⁵

In contrast to the supposed inflexibility of *Orthodoxy in Massachusetts*, Hall's own book, *The Faithful Shepherd* (1972), displays the formulation of Congregationalism in New England as a process of gradual definition, occurring amid a welter of confusion and dispute in which decisions were always shaped as much by political and social needs as by theory. From the days of initial settlement to the Cambridge Platform, Hall has traced the gradual institutionalization of the Congregational order. He has related how the evangelical tone of the early days was superseded by a more formal, sacerdotal quality. But Miller would not have recognized Hall's

³⁴ Hall, "Introduction" to Miller, *Orthodoxy in Massachusetts* (Torchbook edn., New York, 1970), xvi, and "Understanding the Puritans," in Herbert J. Bass, ed., *The State of American History* (Chicago, 1970), 331–32.

³⁵ Hall, "Introduction," xiv, xv, xvi, xviii, and *The Faithful Shepherd: A History of the New England Ministry in the Seventeenth Century* (Norton edn., New York, 1974), 83. Darrett Rutman has vigorously seconded Hall's evaluation of Miller in his review of *The Faithful Shepherd*.

characterization of *Orthodoxy in Massachusetts*. In 1959, as if to nullify the sort of reading exemplified by Hall, Miller flatly declared, "I have shown myself the last to deny that the elaborate scheme of ideas imported in 1630 underwent rapid and often astonishing transformations, as it became adjusted to the unpredictableness of the wilderness."³⁶ Transformation is the keynote of Miller's approach to early New England history. Indeed, it is central to his entire conception of the "meaning of America."

Miller's first book can be properly understood only when it is set within the context of his overview of American history. He best summarized his scheme of American development in an article he wrote in 1954, "The Shaping of the American Character." There he posited that the "open" conditions of American existence exacerbated the existential dread to which all men are subject. The flux, instability, and consequent uncertainty, which are part and parcel of American opportunity, engender a chronic anxiety. In response, Americans have repeatedly endeavored "to fix the personality of America in one, eternal, unchangeable pattern," have tried "to escape from such anxiety by affixing [their] individuality to a scheme of unchanging verities." Yet, as Miller observed, American history "promises no success to the frantic gesture."³⁷ Miller's history is the tale of men who, in the midst of unsettling change, tried to confine their lives within some rigid, ideal pattern, only to witness that pattern crumble before the inexorable movement of life and experience. Vital experience always outran abstract ideals. Although men clung to the forms of the past for emotional security, they had to evolve new identities in the process. Miller did not regard this evolution as a matter of decline. Rather, as in the example of the disintegration of the Puritans' medieval ideal, he saw a progressive, if fitful, growth of human self-awareness and self-affirmation.³⁸

The Puritan example was the first of many instances of this futile tendency to flee from life's vicissitudes into some imaginary realm of static ideals. In that sense it was prototypical. What Miller found supremely interesting about the Puritan enterprise was that "after a century or more of experience on this continent . . . the Puritan colonies found the covenant theory no longer adequate. It broke down because it tried, in disregard of the frontier and a thriving commerce, to stereotype the image of America, to confine it to the Procrustean bed of a priori conception." In this respect, "the New Englanders tried to detach themselves from history and to stabilize their segment of the Revolution"—a goal that was bound to fail when confronted by the exigencies of life. "The American mind discarded the Puritan notion of its personality because the ingenuity required to maintain it was more than men had time or energy to devise."³⁹ Miller certainly emphasized the static, medieval ideals of Puritan thought, but not because he denied the reality of change. On the contrary, he did so precisely to provide a background against which to contrast the results of inevitable change. Although Hall has found "little continuity"

³⁶ Miller, *Orthodoxy in Massachusetts*, xxxiv.

³⁷ Miller, *Nature's Nation*, 13.

³⁸ See, for example, Miller, *The New England Mind: The Seventeenth Century*, 398–400, 449.

³⁹ Miller, *Nature's Nation*, 7, 8, and "The Cambridge Platform in 1648," in Henry Wilder Foote, ed., *The Cambridge Platform of 1648* (Boston, 1949), 72.

in Miller's writings on Puritan New England, they actually form an integrated whole with respect to both theme and method.⁴⁰ Miller's first two books are accounts of the theoretical and practical terms by which the Puritans tried to establish a timeless system. His later volume on *The New England Mind* and the latter portion of *Orthodoxy in Massachusetts* deal with the unavoidable breakdown in their efforts to stop the tide of history. To suggest that religious uniformity was a central ideal of Puritan New England does not imply that such homogeneity was actually achieved. David Hall has confused the ideals of the Puritan divines, as depicted by Miller, with Miller's own understanding of the realities of New England history. Miller demonstrated that the formulation, as well as the dissolution, of the New England Way was fraught with unexpected occurrences.

There can be no mistake about the evolutionary theme of *Orthodoxy in Massachusetts*. The primary purpose of the book was not, as critics have claimed, to revise the old account of the origin of the first Congregational church in Massachusetts Bay by stressing the founders' commitment to Congregationalism prior to emigration despite their abhorrence of separatism.⁴¹ That was only preliminary to showing how the application of the abstract and general ideal of uniformity shifted in practice once the Puritans moved from a position of dissent in England to one of dominance in Massachusetts. Evolution is central, not peripheral, to Miller's story. The great theme of the book is "the transformation of the English heterodoxy into the New England orthodoxy"—that is, the steps, often not anticipated, by which a radical movement gradually changed into a formal establishment. Miller found that once in America "the task of dominating a new environment called upon the system to subordinate the radical insistencies of its youth to the responsibilities of a vested interest. The duty of the Church was no longer to hold aloft a barely attainable ideal of Christian virtue, but rather to train up law-abiding members." At the expense of perfecting "methods for maintaining uniformity" among the churches, the Puritan ministers "had largely transformed the nature of the polity."⁴² What began with utopian ideals had become a legitimized order by the end of the 1640s. Something more than closing loopholes had occurred.

Miller regarded this process as deeply ironical. That "the Reformation was full of incongruities" was amply attested to by what happened in Massachusetts. There,

⁴⁰ Hall, "Introduction," ix. Miller's supposed lack of methodological continuity has been mentioned by a number of commentators. They have pointed out quite rightly that the second volume of *The New England Mind* is developmental and takes up social, economic, and political matters, but they have been unable to reconcile this approach with his alleged static methods of the first volume. Thus, for example, Robert Skotheim wrote that "it is not obvious why Miller told only the story of the dissolution of Puritan thought in terms of interaction with an economic environment"; *American Intellectual Histories and Historians*, 201. Also see Fischer, *Historians' Fallacies*, 198. Even so astute a commentator as Gene Wise has not seen any overall uniformity to Miller's method. Wise has furthered our understanding of Miller's outlook by implicitly showing Miller's pragmatic operating assumptions in what he called Miller's "situation-strategy" approach to ideas. Unfortunately, Wise has tended to accept the usual stereotype as it applies to the first volume of *The New England Mind*. Therefore, he has mistakenly found no comprehensive unity to Miller's methods. For him, the second volume constituted a "potential revolution" in the techniques of intellectual history, when actually Miller did nothing in that book that he had not already done in his first book twenty years earlier. Wise, *American Historical Explanations*, 134, 316, xiii.

⁴¹ Edmund S. Morgan, "Perry Miller and the Historians," *Proceedings of the American Antiquarian Society*, 74 (1964): 11–12.

⁴² Miller, *Orthodoxy in Massachusetts*, 256, 195, 210. Also see *ibid.*, 176, 222.

those “who formerly scorned the social and ethical arguments of the Anglican Church were reduced to defending Congregationalism on those very grounds; the erstwhile rebels against formalism came finally to rest upon formality.”⁴³

These remarks by Miller, among other such statements, are not those of a man who thought the formulation of orthodoxy was merely a matter of duplicating a precise, previously laid-out pattern. The details of the Congregational polity had not been worked out by theorists prior to emigration. Critics may think that Miller cast the background of Congregationalism into “neat categories,” but, in fact, he cautioned that “many, perhaps the greater number, of reputed Puritans entertained but vague conceptions of the discipline.” The early sketches of church discipline by Cartwright, Travers, and Udall “were of a somewhat utopian character” and were accepted by disciples who did not possess “any too concise notions of detailed provisions.” Until 1641 the position of opposition prevented such practical experience as could have sharpened theoretical precision. Miller made the connection between practice and theory unmistakable: “as long as the reform remained primarily a negative movement, as long as the suppressing activities of the government held discussions down, and as long as many individuals became allied with the movement out of antagonism to the Court, the ‘Puritan party’ remained amorphous.”⁴⁴

Miller certainly believed that the theoretical foundations of Congregationalism had been worked out before the Great Migration, but he also plainly argued that the embodiment of Congregationalism in New England was shaped by practice according to the needs of the moment in that new environment. The founders’ “conceptions of polity had hitherto been largely theoretical; many exigencies had not been foreseen, and they preferred not to cross bridges until they came to them.” Although the leaders “were prepared to make their ideals a Procrustean bed upon which society would be stretched or hacked to fit the predetermined dimensions,” they moved carefully and adapted their general goal to the novel conditions.⁴⁵ Of course, they justified their improvisation on the grounds that it was mandated by scripture, but Miller regarded such justification as a rationalization of expediency:

At first, when details of the polity had not yet been worked out, the church covenants cautiously bound the members merely to walk in the ways of the Gospel and “in all sincere

⁴³ *Ibid.*, 65, 198. Hall noted Miller’s ironic theme, but he failed to connect it to Miller’s evolutionary perspective. Hall, “Introduction,” xxii. Irony is a pervasive theme in Miller’s writings from the beginning. Gene Wise, however, has argued that only with the second volume of *The New England Mind* did Miller’s use of irony as a paradigm emerge, and he has maintained that the use of irony is a token of the radical change in Miller’s methods that occurred between the appearance of the first and the second volume. The prominence of irony in *Orthodoxy in Massachusetts* shows Wise’s argument to be untenable. Even in the first volume of *The New England Mind* irony is central. Although Wise found “few ironies” in that book, Miller indicated the importance of his ironic theme when he observed that “he who undertakes to narrate any chapter in the history of the Reformation must have an appetite for paradox and an appreciation of the ironic”; *The New England Mind: The Seventeenth Century*, 181. Miller also made the same point just as explicitly in his early article, “The Puritan Theory of the Sacraments,” 418. For Wise’s views, see his *American Historical Explanations*, 333–34, and “Implicit Irony in Perry Miller’s *New England Mind*,” 597.

⁴⁴ Miller, *Orthodoxy in Massachusetts*, 73, 74. Also see *ibid.*, 80. For the reference to “neat categories,” see Hall, *The Faithful Shepherd*, 23.

⁴⁵ Miller, *Orthodoxy in Massachusetts*, 148, and “The Half-Way Covenant,” *New England Quarterly*, 6 (1933): 676.

Conformity to His holy Ordinances." All developments of the next eighteen years were assumed, nevertheless, to have been implied in those covenants, and additions or improvements were continually found justified by holy ordinances.

The leaders' ideal was conceived as a timeless, divinely preordained system, but it was in reality achieved by practical accommodation. Despite their a priori frame of mind, which disposed them to be "sublimely indifferent to what might be the practical consequences of behavior," when it came actually to establishing a holy commonwealth Miller's Puritans could be "shamelessly pragmatic."⁴⁶ Theirs was an unacknowledged pragmatism serving the interests of an a priori goal.

Miller did not "overlook . . . the freedom that the Holy Spirit brought the saints."⁴⁷ On the contrary, his recognition of the Puritans' spiritual restlessness has become apparent. "Up and down" seventeenth-century England "were men with utopias in their brains and the voice of God in their ears," and the zeal of this "often cantankerous crowd of erstwhile dissenters" had to be restrained if a stable commonwealth were to be achieved. "The Massachusetts experiment would have been shattered had the centrifugal forces of Protestantism broken loose." Thus, the founders of Massachusetts were engaged with the problem of balancing the religious enthusiasm of the settlers with the need for social order. Both had to be preserved if their goal was to be realized. *Orthodoxy in Massachusetts* is a story of the difficulties (and ultimate impossibility) of institutionalizing an ecstatic religious experience. Here was an important source of political conflict. While dissent was ascendant in England, conformity became more important in New England. Once there, the ministers "went over the whole framework of Congregational thought, tightening up the bolts of cohesion." Therefore, as Congregationalism "became a system of control in New England, the emphasis inevitably shifted."⁴⁸

A careful comparison of Miller's framework for interpreting the events of early Massachusetts with the conceptual scheme of David Hall reveals substantial similarity. The theme of the routinization of religious zeal is central to both. Both historians depicted the Puritans as seeking to hew a middle way between an empty formalism and a chaotic spiritualism, and both showed a growing authoritarianism in New England practice, designed to counter disruptive religious fervor and to perpetuate what had already been accomplished. Miller's history is as dynamic, pluralistic, and realistic as Hall's.

If Hall has not been able to break loose from the outlines of Miller's true historical scheme, neither has Robert G. Pope. Pope's attack on Miller's "myth of declension" constitutes a particularly sharp example of the way in which Miller has been pommelled by his own arguments in the hands of revisionists striving for originality. Pope has continued the standard position that Miller subordinated external behavior to internal thought in his approach to Puritanism. With Miller as the implicit target, Pope admonished the intellectual historian to remember that,

⁴⁶ Miller, *Orthodoxy in Massachusetts*, 160, "The Half-Way Covenant," 676, *Errand into the Wilderness*, 88, and *The New England Mind: The Seventeenth Century*, 395.

⁴⁷ Hall, "Introduction," xvii.

⁴⁸ Miller, "The Half-Way Covenant," 676, and *Orthodoxy in Massachusetts*, 176, 162, 176–77, 187.

when he “attempts to interpret social change,” he “must occasionally look to the external realities.” By ignoring them, “the basis for the critical judgment of ideas is lost.” Pope has asserted that, because Miller neglected the social facts and tried “to extrapolate social history from intellectual sources,” he hatched “some misconceptions about New England.” Miller was so “swayed by the Puritans’ rhetoric” that he “lost track of New England’s realities.”⁴⁹

In particular, Pope has suggested that Miller’s undue ideational focus upon the ministers’ jeremiads of the later seventeenth century led him to accept their lamentations at face value. Like the jeremiads, Miller’s “portrait of declension” is governed by the assumptions that “at some point the Puritans achieved an ideal intellectual and social system” and that “church membership steadily declined in the last three decades of the century.” To counter this supposed decline, the ministers devised the half-way covenant.⁵⁰ Pope implied that Miller accepted without question “the thesis that the churches immediately filled with half-way members” in response to the innovation and that ultimately this measure “contributed to further attenuation of religious zeal.” Pope has branded Miller’s account of declension “totally inaccurate.” As an example of how Miller lost touch with the facts of New England history, Pope cited Miller’s enshrinement of Solomon Stoddard in “New England’s pantheon for his supposed role in revitalizing Congregationalism” by opening “the church to all godly inhabitants,” regardless of whether they had undergone “the regenerative experience.” Pope has found, however, that Stoddard was anticipated by a number of ministers in Connecticut.⁵¹

Pope has called for a new interpretive approach to the changes in Congregation-

⁴⁹ Pope, “New England versus the New England Mind: The Myth of Declension,” *Journal of Social History*, 3 (1969–70): 97, 96. Pope’s remarks echo the position taken by Bernard Bailyn in his review of the second volume of *The New England Mind* in 1954. Bailyn implied that Miller’s history, brilliant though it was, did not have adequate grounding in the realities of Puritan society. At the time of the paperback republication of *The New England Mind* in 1961, Miller addressed himself to Bailyn’s characterization of his book. He vehemently denied that his account “was floating in thin air, like some insubstantial island of Laputa”; Miller, “Preface to the Beacon Press Edition,” in *The New England Mind: From Colony to Province*.

⁵⁰ The half-way covenant refers to the propositions of the Synod of 1662, called by the General Court of Massachusetts to resolve the question of who was subject to baptism. In early Congregational practice, church membership was limited to those adults who voluntarily could make a public profession of the working of saving faith within themselves. If the church was satisfied as to the authenticity of their experience, they were admitted to church membership and received the sacraments of baptism and the Lord’s Supper. In addition, upon scriptural authority, the children or “seed” of such persons were deemed to be within the church covenant. They received baptism and were expected upon reaching adulthood to make their own profession of saving faith, which would admit them to the Lord’s Supper and entitle their own children to baptism. The problem arose from the unexpected, but increasing, failure of the second generation to experience conversion as they attained maturity. Short of conversion, would they retain their membership and could their own children in turn be baptized? The question was important because upon it hung the fate of Congregationalism and, more generally, of New England’s mission. To exclude from baptism children whose parents failed to proceed to conversion threatened the continued existence of the churches; to include them would adulterate the churches’ purity. In the end a majority of the ministers sacrificed purity to ensure institutional survival. The grandchildren and succeeding generations were to be admitted to the church and baptized, but they were excluded from the Lord’s Supper until they could testify to their own regeneration. Though within the church, these members were, so to speak, only half-way on the path to sainthood. In practical terms, two classes of membership were established. This policy, which had been developing informally for some years, was officially adopted by the Synod of 1662, despite spirited opposition, and it received the imprimatur of the Massachusetts General Court.

⁵¹ Pope, “New England versus the New England Mind: The Myth of Declension,” 97, 101, 96. Also see Pope, *The Half-Way Covenant: Church Membership in Puritan New England* (Princeton, 1969), 96–131, 255–57, 268–69.

alism—one that combines the “church-sect” typology of Ernst Troeltsch, Max Weber, and others—with “functional analysis.” Employing his sociological perspective, Pope has found that New England Congregationalism underwent a difficult transition from the ideals of a sect to the institutions of a church. According to him, “the religious history of seventeenth-century Massachusetts is the story of the struggle to live with a concept of purity within the context of a covenanted community.” The failure of the children of church members to convert confronted the churches with “an unpleasant dilemma: they could either bring themselves more fully into the community, or they ran the risk of losing control completely.”⁵²

The power of the Miller stereotype obscures Pope’s perception of Miller’s developmental approach and rather forthright institutional themes. Miller did not assume that with the Cambridge Platform ideal became fact. He did not take literally what the ministers bewailed, nor did he neglect the social realities. Given his evolutionary, experimental account of the establishment of Congregationalism, the Cambridge Platform symbolized to him a futile effort to stop the wheel of history by “affixing” the New England identity “to a scheme of unchanging verities.” In this context the jeremiads betokened the stressful changes of an expanding society, changes that could not easily be incorporated into the Puritan’s ideal conception of themselves. The jeremiads were in part an index of the psychological agony the Puritans suffered as New World opportunity evoked behavior that breached their fragile fortress of a priori identity. Miller did not believe the Puritans had become morally corrupt. Rather, he saw New England evolving, however unwillingly, a new definition of self:

But as the rewards [from “pious industry”] came in, and New England adjusted itself to different circumstances, it was perforce compelled to take cognizance of other matters than sanctity and polity. The truth of the matter seems to be not that New England was declining but that it was changing; it had become something other than it had started out to be, in spite of the fact that many who were responsible for the change still desired with all their hearts that it remain unchanged.⁵³

Miller’s critics have often interpreted his theme of the waning of Puritan piety to mean a decline of religiosity in response to a growing secularization of society. This is a serious error because it confuses the decline of only one kind of piety with religiosity as a whole. When Miller spoke of the waning of piety, he always meant the fading of the particular, acute Augustinian sense of anguish and elation that occurred as Congregationalism became “familiar custom and established order.” He was not speaking of a comprehensive weakening of religion. In keeping with his theme of the evolution of heterodoxy into orthodoxy, he depicted the inevitable ebbing of the intense enthusiasm of a radical movement as it became formalized routine. This was the reason for fewer conversions among the second generation. The failure of the children to convert was part of the “old and familiar” story of

⁵² Pope, “New England versus the New England Mind: The Myth of Declension,” 103–05. Also see Pope, *The Half-Way Covenant*, 261–78.

⁵³ Miller, *Nature’s Nation*, 43.

“what happens to a successful revolution.” Miller cautioned against using the piety of the founders as the exclusive benchmark for measuring the spiritual pulse of later generations. To be sure, when “compared with the founding generation there had been a notable falling off and lessening of zeal” by the early eighteenth century. Yet, “even in 1720 or 1730, when the ministers thought the ‘declension’ was at its lowest, the New England communities were, judged by the ordinary standards of life today, sufficiently pious.”⁵⁴ What Miller’s history relates is not the flagging of religion but a shift in its tone. New Englanders remained a God-fearing people even though the special zeal of the early years slackened.

Miller did write, then, about developments in the New England economy during the later seventeenth century, which diverted attention from the high ideals of the founders. As he tersely put it, “pious industry wrecked the city on a hill.” This was not, however, a simple case of growing irreligion or secularism, a matter of religious interests losing out to economic opportunity. Miller’s point is ironical. Behind economic activity was pious intention, and the unforeseen consequence of that intention was a weakening of the original utopian ideals. Industry was mandated as a way to glorify God, yet the fruit of industry distracted the Saints from their initial errand. Success bred failure. The jeremiads registered the implicit conflict between the pious motives of both the work ethic and the original mission. “They were professions of a society that knew it was doing wrong, but could not help itself, because the wrong thing was also the right thing.”⁵⁵ The very vehemence of the jeremiads, as portrayed by Miller, is ample testimony to the continuing vitality of religion in New England.

If Miller did not subscribe to the “myth of declension,” neither did he see a steady decline in church membership that was dramatically reversed by a flood of new half-way members. Miller’s position is thus little different from Pope’s contention that “neither churches nor individuals flocked to take advantage of more generous baptism.”⁵⁶ According to both, the ministers were the main proponents of the half-way measures, while opposition centered in the laity and in a minority of the clergy. Miller noted “the doggedness of the opposition,” remarking that “it took the clergy thirty years or more to persuade their flocks” to accept the half-way covenant. Although Miller pointed out that “the ministers were obliged . . . to fill up the churches with obedient citizens, even though they had to search for them outside Israel,” he did not describe actual accomplishment; his point, rather, indicates ministerial purpose and subsumes other measures, beyond the half-way covenant, taken by the clergy to bolster membership.⁵⁷ In addition, Miller explained why half-way members failed to proceed to full ownership of the covenant (and it was not simply a matter of growing secularization, as Miller is supposed to have argued): on the one hand, half-way members sought to avoid the social responsibilities full ownership entailed; on the other, some were embroiled in the “dilemma of the

⁵⁴ Miller, “Puritan Theory of the Sacraments,” 410, and *Errand into the Wilderness*, 158.

⁵⁵ Miller, *The New England Mind: From Colony to Province*, 49, 51.

⁵⁶ Pope, “New England versus the New England Mind: The Myth of Declension,” 99.

⁵⁷ Miller, *The New England Mind: From Colony to Province*, 109, 105, 113, and “Solomon Stoddard,” 290.

sacrament.” In other words, the doubts of the scrupulous kept them from making a full profession of their faith, which is precisely what Pope has argued.⁵⁸

Pope’s work is one of amplification, not innovation. Virtually all of the elements that compose his revised reading of the changes within the church system of New England were earlier made or adumbrated by Miller. Miller was familiar with Troeltsch and Weber.⁵⁹ What Pope describes as the “inherent tension and conflict between the goal of a covenanted community and that of the covenanted churches” is a basic theme of both *Orthodoxy in Massachusetts* and the second volume of *The New England Mind*. Troeltsch’s “church-sect” typology is implicit in Miller’s account of “the transformation of Congregationalism from a religious Utopia to a legalized order.”⁶⁰ Miller’s Puritans adopted “many ideas not so much for theological as for social and economic reasons,” and his analysis of the debate over the half-way covenant bears out this pragmatic interpretation. He understood the problem posed by the failure of conversion among the second generation as a matter of institutional survival wherein “social expediency” was in conflict with “religious tradition.” If the churches were “to retain their pristine insistence upon the reality of spiritual life as the sole basis for church life,” they could expect to “watch the membership dwindle.”⁶¹ The “crucial weakness” of the opponents of extended baptism was that their position “would have reduced the churches to a wraith.” The issues raised by the unconverted children were a “most serious threat” to the New England Way. The danger was civil as well as ecclesiastical, because without the discipline of the churches “the regime would not survive.” Thus, the proponents of liberalized baptism prevailed “on the score of practical effectiveness.” As Miller read it, the “moral” of the dispute was that, although Puritans always had to justify themselves on the a priori grounds of scripture, “yet as between one array of proof and another, they could find it convenient to choose that which was the more compatible with their mundane interests.”⁶²

Pope has made a genuine contribution to scholarship by revealing the rich diversity of church practice in later seventeenth-century New England. In particular, he has illustrated the variety of “presbyterial” procedures developed in Connecticut to broaden church membership beyond the provisions of the half-way covenant.⁶³ Since these practices anticipated the reforms of Solomon Stoddard in Northampton by several years, Pope has argued that Miller’s “revolutionary” label

⁵⁸ Miller, *The New England Mind: From Colony to Province*, 126–27, 224–25, 277–78, and “Solomon Stoddard,” 296–97, 308–09; and Pope, “New England versus the New England Mind: The Myth of Declension,” 99. John M. Murrin has accepted Pope’s criticism of Miller without question: “until the last decade, the only explanation for the decline of conversions leading to the Half-Way Covenant was a presumed secularization of the second generation”; and he identified Miller’s second volume of *The New England Mind* as one work in which this assumption is prominent; Murrin, “Review Essay,” *History and Theory*, 11 (1972): 235.

⁵⁹ See, for example, Miller, *Orthodoxy in Massachusetts*, 15, 270, *The New England Mind: The Seventeenth Century*, 43, and *The New England Mind: From Colony to Province*, 41.

⁶⁰ Pope, “New England versus the New England Mind: The Myth of Declension,” 103; and Miller, “The Half-Way Covenant,” 703.

⁶¹ Miller, *Orthodoxy in Massachusetts*, 205, 202–03.

⁶² Miller, “The Half-Way Covenant,” 710–12. Also see *ibid.*, 699.

⁶³ Pope was careful to distinguish presbyterialism from Presbyterianism. For him, the presbyterialist occupied “a half-way house between Congregationalism and Presbyterianism.” What presbyterialists sought was “a more inclusive form of church membership that would bring more people into the church,” even though “the expanded membership was not always admitted to full communion.” They may have “recognized the validity of

for Stoddard's innovations is unwarranted. In taking Miller to task for ignoring what had already happened in Connecticut, Pope has attributed Miller's oversight to his supposed ideational tunnel vision.

Pope's analysis is not, however, entirely fair to Miller's interpretation. Miller did not detail the presbyterial developments of Connecticut, but he was not unaware of them. In the second volume of *The New England Mind*, Miller mentioned them, and he also provided an explanation of the logic behind them. Miller noted that in 1680 the Connecticut General Court lamented the diversity of church practice in the colony, which had arisen from the disputes of the previous fifteen years. The Court wrote that "some of them [are] strict Congregationall men, others more large Congregationall men, and some moderate Presbyterians." In Miller's account, presbyterialism in Connecticut was an aspect of the "social evolution" of the period. The New England identity as a covenanted people was becoming endangered by the "impulses of a new society," which gave rising influence to new men of secular affairs who were not necessarily part of the church. To maintain church and social order, as well as the society's dedication to holy ideals, it was necessary to include these persons within the "net of obligation." The erosion of the churches' influence prompted some ministers to extend the pragmatic logic behind the half-way covenant to include those with no formal connection to the churches. For this reason, "around 1680 (in some localities even earlier) leaders of the half-way principle found themselves prepared to accept into half-way membership persons hitherto outside the covenant—persons formerly considered unregenerate but who now were ready to make the same acknowledgment of obligation as was demanded in an 'owning of the covenant.'"⁶⁴ To preserve the ideals of a holy commonwealth, ministers were coming to sacrifice the ideal of a covenanted church.

Miller recognized that the practice of extended baptism had grown gradually in Connecticut over many years. In the early 1660s the General Court of Connecticut, in what was a radical proposal for the time, queried "the ministers whither it be not their duty to enterteine all such persons, whoe are of an honest and godly conversation, haveing a competency of knowledg in the principles of religion, and shall desire to joyne with them in church fellowship."⁶⁵ Miller's comments on this proposal show his clear appreciation of the social and ecclesiastical trends in the colony:

The tentative recommendation of Connecticut's General Court, which in 1664 seemed too shocking even to contemplate, was imperceptibly accepted by church after church, until by 1684 the general practice had become to receive into half-way status any who were of decent conversation and showed a competency of knowledge, who would join the inferior segment of the ecclesiastical society in order to gain baptism for their children.

There is no suggestion on Miller's part of Stoddard's priority in these affairs. The plight of the churches evoked similar, but separate, responses. The chaos in

standing synods and highly structured interchurch relations," but they were primarily interested in "transferring control of church affairs from the hands of the laity into the hands of the elders." Pope, *The Half-Way Covenant*, 76 n. 1.

⁶⁴ Miller, *The New England Mind: From Colony to Province*, 106, 112–13.

⁶⁵ Quoted in Pope, *The Half-Way Covenant*, 77.

Connecticut during the late 1660s caused those advocates of the half-way covenant, dubbed "Presbyterian" by their opponents, "to wonder if perhaps, after all, Presbyterianism might not better deal with anarchic propensities." For the same reasons, "in Northampton, new thoughts began to work in the mind of Solomon Stoddard, summoned in 1669 to succeed Eleazar [Mather] in a valley reverberating with Connecticut's quarrels."⁶⁶

Miller did, of course, give priority to Stoddard for being the first "openly" to scrap the half-way covenant and to take "into the church and up to the Lord's Supper everyone who would or could come." Miller judged Stoddard's act to be revolutionary because it overthrew the concept of a covenanted church. Pope has claimed that several years earlier a few Connecticut ministers had already done substantially the same thing. Even here on Pope's strongest point, however, the issue of priority seems moot. Pope's exposition does not make it entirely clear whether the Connecticut ministers had gone as far as Stoddard did. John Woodbridge, Jr., the minister in Killingsworth from 1666 to 1679, was "the most successful advocate of the presbyterial polity." Woodbridge still personally examined the qualifications of candidates for membership, so that, although he "undermined the congregation's role in determining an individual's fitness for communion and assumed the obligation himself, he did not, it would seem, admit persons indiscriminately."⁶⁷ Miller, however, noted that Stoddard "took into the communion not merely the professing adults, but all adults." Furthermore, Miller's use of the word "openly" to describe Stoddard's renunciation of the half-way covenant implies a background of practice on the part of other ministers that did not receive the publicity Stoddard earned. Whether or not Stoddardeanism is judged to be revolutionary is a matter of perspective. Perhaps his reforms were not extreme when set against Connecticut's precedents, but from the standpoint of the New England Way and contemporary practice in Massachusetts, they were radical, as Pope himself conceded.⁶⁸ In any event, Miller's interpretation of Stoddard offers no grounds for supposing that he ignored external reality and extrapolated social history from insubstantial ideas. Upon close examination, Pope's differences from Miller seem rather small.

Much the same can be said of other revisionists. There are, for example, remarkable parallels between Sacvan Bercovitch's recent reassessment of the Puritans' errand and jeremiad and Miller's writings on the same subjects. In his preface to *The American Jeremiad*, Bercovitch disclaimed association with the "swarm" of critics who "rushed" to dismember Miller's work after his death. Yet Bercovitch's critique has all of the earmarks of revisionism. He has seriously misconstrued Miller and then offered as an alternative an interpretation that is in fact quite similar to Miller's. Like Pope, Bercovitch assumed that Miller subscribed to the reality of declension. With the failure of their initial errand, the Puritans turned inward only to find a "sink of iniquity." The Puritans "had been twice

⁶⁶ Miller, *The New England Mind: From Colony to Province*, 113, 106.

⁶⁷ Miller, *Errand into the Wilderness*, 160, and "Solomon Stoddard," 298; and Pope, *The Half-Way Covenant*, 102, 103.

⁶⁸ Miller, "Solomon Stoddard," 298; and Pope, *The Half-Way Covenant*, 255.

betrayed. Not only had the world passed them by, but the colony itself, the city set on a hill as a beacon to mankind, had degenerated into another Sodom." Therefore, Miller viewed the jeremiad primarily as an outcry of despair. Bercovitch has contended that in stressing "the dark side of the jeremiad" Miller offered only "a partial view of their message." What he missed was the accompanying note of optimism that gave the jeremiad an intrinsic ambivalence. In ostensible contrast to Miller, Bercovitch has focused "on the affirmative energies" of the jeremiad. The "essence" of the jeremiad was "its unshakable optimism." In reality, "the jeremiads attest to an unswerving faith in the errand" of the Bible Commonwealth. They were a device to "revitalize" it. For this reason, "the most severe limitation of Miller's view is that it excludes (or denigrates) this pervasive theme of affirmation and exaltation."⁶⁹

Contrary to Bercovitch's analysis, Miller's portrait of the "classic" jeremiad does indeed stress its ambivalence—its optimism as well as its pessimism. Its pessimism was a symptom of the profound identity crisis that beset Puritan New England in the second half of the seventeenth century. This crisis had two sources. In the aftermath of the English Civil War, New England suffered a "shattering blow to [its] self-esteem" when in the home country the ultimate purpose of its mission "was rejected by those choice spirits for whom the exertion had been made." The Puritans were overcome by an agonizing "sense of the meaning having gone out of life" and were therefore burdened by the painful task of having to redefine themselves.⁷⁰ In addition, there were the distressing changes of an expanding society, which threatened traditional values. The jeremiads registered the anguish of a society that was clinging to the past while it was forced to reforge its self-conception.

In Miller's account, however, the jeremiad was more than a wailing of tormented souls. As portrayed by him, its ultimate, if unrecognized, purpose was to serve as an engine of hope and activity. The jeremiad was a form of ritual confession, the consequences of which seem paradoxical. As a recognition of guilt, confession is a rededication to the values that have been violated, but at the same time the absolution that confession confers restores the soul's vitality, which in turn abets renewed transgression. As Miller put it, the purgation of the soul administered by the jeremiad will "actually encourage the community to persist in its heinous conduct."⁷¹ Furthermore, a chosen people will interpret affliction as a divine reprimand, and thus as a sign of a continuing divine solicitude that reconfirms the covenant. Of course, in the covenant's economy of rewards and punishments, no one could be certain that the enormity of society's sin had not broken the bond. One was left in doubt, suspended between hope and despair. Unsettling, chronic doubt, however, prompts one to action, to public confession. Therefore, "a day of humiliation would be a device both for regaining confidence and for reasserting, in

⁶⁹ Bercovitch, *The American Jeremiad*, 5, xiv–xv, 6–7. The first chapter of Bercovitch's book, which reassesses Miller and the jeremiad, is published in abbreviated form in Higham and Conkin, *New Directions in American Intellectual History*, 85–104. In the version in *New Directions*, Bercovitch's belief that Miller accepted at face value the ministers' complaints is most explicit: "Miller's argument, it is worth noting, essentially follows from the lament of the Old Guard Puritans, who, like Danforth, bewailed their alienation from their times. No doubt they felt alienated, but we need not simply accept their view of the situation, as Miller does." *Ibid.*, 104 n. 18.

⁷⁰ Miller, *Errand into the Wilderness*, 11, 13, 14.

⁷¹ *Ibid.*, 9.

the face of adversity, an assurance that felicity exists." Even in the "worst of afflictions," a covenanted people could find "consolation" because "afflictions signify the presence of God."⁷² The jeremiad was, therefore, an important component in Puritan New England's institutionalization of the bite of conscience. Jeremiads aggravated the sense of guilt, focused it, and then provided the means of relief through the appropriate channels of repentance and pious labor. As delineated by Miller, the psychological mechanism of the jeremiad operated on the principle that doubt can be an important prod to activity.

Far from placing a one-sided emphasis on the jeremiads' pessimism, Miller's interpretation fuses anxiety with exertion and hope. In words that unequivocally contradict Bercovitch, Miller wrote that the jeremiads were "necessary releases":

they played a vital part in the social evolution because they ministered to a psychological grief and a sickness of the soul that otherwise could find no relief. . . . They were social purgations, enabling men to make a public expiation for sins that they could not avoid committing, freeing their energies to continue working with the forces of change. . . . From such ceremonies men arose with new strength and courage. . . . They . . . thus regained something of their self-respect, though paradoxically they had to acquire it by confessing their iniquities.

The ultimate effect of the jeremiad was that the Puritans were "more prepared to march into the future." Puritan ministers only "half understood" what they were doing, but in their hands the jeremiad became "an engine of Americanization," which made "intelligible order out of the transition from European to American experience."⁷³ As early as 1938, Miller noted that "when the seventeenth-century preacher wanted to arouse men he would . . . instance afflictions already suffered, or predict those to come, . . . punishments that men might survive and from which they might profit." Thus, their "sermons stress continually the note of hope, the possibility that anyone, no matter how immoral or depraved he has been, may yet be saved."⁷⁴ Miller's testimony is incontrovertible. No less than Bercovitch, Miller recognized the affirmative energies of the jeremiad.

WHAT IS REMARKABLE ABOUT REVISIONIST CRITICISM is its failure to appreciate Miller's philosophical assumptions and, consequently, to perceive the existential themes underlying Miller's conception of the meaning of America. The revisionists betray little feeling for the movement in Miller's history, or for its comprehensive

⁷² Miller, *The New England Mind: From Colony to Province*, 22, 24–25. Also see Miller, *Errand into the Wilderness*, 120.

⁷³ Miller, *Nature's Nation*, 48–49, and *The New England Mind: From Colony to Province*, 40, 26, 31.

⁷⁴ Miller, *The Puritans*, 289. The power of the jeremiad to stimulate activity is most apparent in Miller's description of the jeremiad's use during the Revolutionary War. According to him, the refurbished jeremiad of this period was the real engine of patriotic resistance. For the rank and file, opposition to Britain did not stem primarily from the persuasion of abstract theory. Instead, the patriotic will was stiffened by resort to the traditional Puritan "rite of self-abasement." In the patriotic jeremiads there was a "coalescence of abnegation and assertion." They served as "a dynamo for generating action." As Miller put it, their "identification of Protestant self-distrust with confidence in divine aid, erected a frame for the natural rights philosophy wherein it could work with infinitely more power than if it had been propounded exclusively in the language of political rationalism." Miller, *Nature's Nation*, 100–01. Miller's approach to the Revolutionary jeremiads demonstrates well his general assumption that abstract ideas are usually espoused not for some inherent logic but for more basic spiritual or practical purposes.

thematic unity. All too often their criticism wrenches Miller's words out of context. They mistakenly concentrate on his themes of stasis, resistance to change, and decline and ignore his equally prominent themes of transformation, growth, and advance.

Miller is often thought to have been hostile to the modern world. Themes of growth and development, it is said, were uncongenial to his temperament. His outlook on the course of modern Western history has been characterized as nonprogressive. John Higham, for example, declared categorically that Miller operated "without guidance from a theory of progress or any other general scheme of historical development." Speaking of Arthur O. Lovejoy, Ralph Gabriel, Samuel Eliot Morison, and Perry Miller, Higham claimed that they, in contrast to the New Historians, "had never expected the course of history to vindicate their values. They did not imagine their own world—or what they could anticipate in the future—to be any great improvement over the past they wrote about. Consequently they escaped the progressive historians' tendency to appraise ideas instrumentally." Henry F. May's views match those of Higham. May has maintained that "Miller was deeply and even agonizingly ambivalent" about "themes of triumph." According to him, "Miller never felt easy with the Enlightenment, and much of his best work is a polemic against its assumptions." Therefore, "the flow of time, in Miller's best books," is "channeled into stories of decline."⁷⁵ Read in this way, Miller's greatest work, *The New England Mind*, appears to be a lament over the disintegration of a once magnificent holy commonwealth, in which the noble piety of the founders, described in the first volume, is contrasted with the spiritual flabbiness of later generations, as set forth in the second.

This line of interpretation betrays a very imperfect comprehension of Miller and his work. If anything, it stands Miller on his head. To be sure, Miller's writing is set against the assumptions of the Progressive historians, as Gene Wise has explained.⁷⁶ Yet, Miller was not antagonistic to progress, nor did he despair of man's powers of reason. Quite the contrary, all of his work is set against the backdrop of his grand progressive scheme of the gradual, fitful, and painful "transformation of western European culture from a medieval to a modern . . . conception of life and society." Although he was somewhat dismayed by the decline in what he held to be the realistic Augustinian appreciation of human limitations, he was a modern liberal pluralist, and he therefore cast a favorable eye upon the growth of the modern spirit.⁷⁷ Miller was not a utopian. He did not regard history as inexorably moving in some preordained direction, irrespective of human intentions. He did not believe in human perfectibility or in a secular paradise, created by science, that would automatically resolve human problems and ensure happiness. Still, he looked upon the progressive development of modern mind as being an undeniable fact.

⁷⁵ Higham, *Writing American History: Essays on Modern Scholarship* (Bloomington, Ind., 1970), 66, 65; and May, "Perry Miller's Parrington," Review of Miller, *The Life of the Mind in America* (1964), in the *American Scholar*, 35 (1966): 564, 562.

⁷⁶ Wise, *American Historical Explanations*, 296–359.

⁷⁷ Miller, *Errand into the Wilderness*, 100. For examples of Miller's celebration of the modern liberal pluralist spirit, see his "Dr. Conant Graduates: Harvard to Bonn," *Perspectives: USA*, no. 4 (1953): 84–85, "The Reimportation of Ideas," in Bertrand Russell, ed., *The Impact of America on European Culture* (Boston, 1951), 84–85, and *Responsibility of Mind*, 96, 193–94.

Miller understood "errand into the wilderness" to be a metaphor for modern man's agonizing, yet exhilarating, pilgrimage into a consciousness of his existential freedom, an ordeal that he believed has been felt with special poignancy in America. The gradual shift in emphasis within the Puritan errand—from a task externally commanded by God to an enterprise man must determine for himself—symbolized to him the larger transformation in Western culture from the medieval to the modern mentality, in which men have slowly been coming to recognize that upon man alone rests the awesome burden of setting his own ends and purposes in life. To Miller, the evolution of the Puritan errand betokened the growing maturity of Western culture. From an immature "errand boy" man is rising to the stature of being his "own boss." In this respect, Miller shared with Kant the view that men were becoming enlightened, that they were emerging from their "self-imposed nonage" and daring to know.⁷⁸ What intrigued Miller was the question of how much courage modern man would display in facing up to the responsibility of being on his own without the consolation of an appeal to transcendental support.⁷⁹ And from this perspective Miller interpreted the American experience. He saw in American history a great and abiding moral drama in which Americans have been profoundly ambivalent about the responsibilities of freedom. Although they have eagerly pursued freedom's opportunities, they have also been sorely tempted to escape from its uncertainty and insecurity. At bottom, Miller located the meaning of America in the ordeal of freedom. America stood for the Promethean destiny of modern man, and Miller wanted to know whether Americans had the force of character to live up to that calling in which man, with full self-awareness, must be his own master.⁸⁰

⁷⁸ Miller, *Errand into the Wilderness*, 3; and Immanuel Kant, "What Is Enlightenment?" (1784), trans. Peter Gay, in Gay, ed., *The Enlightenment: A Comprehensive Anthology* (New York, 1973), 384. This comparison between Miller and Kant is only meant to show a similarity of attitude toward the fearless use of the intellect, which Kant regarded as being the essence of the Enlightenment's spirit. It is not intended to suggest that Kant and Miller shared similar metaphysical assumptions. For remarks that indicate Miller's positive attitude toward the liberating spirit of the Enlightenment, see Miller, "New England's Transcendentalism: Native or Imported?" in Carroll Camden, ed., *Literary Views: Critical and Historical Essays* (Chicago, 1964), 125, and "On 'The Irresponsibles,'" *Nation*, June 1, 1940, p. 681.

⁷⁹ A recurrent theme in Miller's writings is the terror of the void evoked in early modern man by the revelations of the new telescope. The "mute spaces" of the infinite cosmos impressed sensitive men with their cold, friendless inhumanity. The telescope brought men face to face with their weakness and finitude in the awesome wilderness of the heavens. Miller linked this theme to the Augustinian sensibility and connected both to modern man's sense of alienation in an uncertain world where God is dead and even the laws of science no longer seem secure. See Miller, *Errand into the Wilderness*, 222, 228, *Nature's Nation*, 131–33, *Jonathan Edwards*, 147–49, and *The New England Mind: From Colony to Province*, 440.

⁸⁰ The ordeal of freedom is the most basic of Miller's themes. It is at the heart of his understanding of the meaning of America. He was influenced by Reinhold Niebuhr in part because of Niebuhr's understanding that "both man's misery and man's dignity have the same root, namely, his freedom," Miller, "The Influence of Reinhold Niebuhr," a review of Niebuhr, *Pious and Secular America* (1958), in the *Reporter*, May 1, 1958, p. 40. This same theme can be seen cropping up in the introductions and conclusions of Miller's books, and his *Jonathan Edwards* is devoted in its entirety to the exploration of the problem of freedom. For Miller, Edwards "was a Puritan who would not permit mankind to evade the unending ordeal and the continuing agony of liberty." In that respect, Edwards succeeded as well as any other American "in generalizing his experience into the meaning of America." *Jonathan Edwards*, xiv. Miller was also impressed by Roger Williams because he was a courageous "explorer into the dark places, of the very nature of freedom." Miller, *Roger Williams: His Contribution to the American Tradition* (1953; reprint edn., New York, 1962), 255. Also see Perry Miller, ed., *The Complete Writings of Roger Williams*, 7 (New York, 1963): 24. What Miller admired in both of these men was their existential appreciation of the human condition, which he regarded as very modern.

Miller was therefore a thoroughgoing humanist. What man is, and consequently what man's history is, is a product of human decisions, of what man makes of himself.⁸¹ Miller was not a dogmatic rationalist. He keenly appreciated the disproportion between the breadth of the universe and man's meager powers of mind. Even so, he believed that if man is to live without illusion, he has nothing else but his own reason and intelligence upon which to rely. This situation places a great burden upon him, but for that very reason it is also the source of human greatness. Miller was acutely aware of man's propensity to flee from his existential responsibility, yet, ultimately, he was convinced that men would rise to the challenge. Perhaps his strongest celebration of man's greatness came in an address entitled "Experiences of an American Professor at a Dutch University," delivered in 1951. His conviction that man is able rationally to govern himself is most apparent. Shortly before his talk, Edmund Wilson had reviewed the latest volume of André Malraux's *The Psychology of Art*. Quoting from Wilson's paraphrase of Malraux, Miller said, the meaning of modern art is that man is no longer concerned with arranging his relations with God or with nature, but that he can now assert his right "as a human being to deal with the world about him in an audacious and cavalier way, and to conceive—that is, to organize—his environment, his relationships, and his destiny according to any system he pleases. The universe is there to be used by him, not he by it."

Miller also quoted from William Faulkner's speech accepting the Nobel Prize in 1950. Faulkner believed that "man will not merely endure, he will prevail." Miller praised the declarations of both Malraux and Faulkner for being "two ringing statements" that seemed to him "to speak for the spirit with which we must confront the modern world." He felt that "it is of the greatest importance that educators, whether in Europe or in America, be consistently aware of this necessity." Nothing could be further from the truth than John Higham's assertion that Miller believed man to be existing in a universe that he "cannot master or more than superficially comprehend."⁸²

Miller reached intellectual maturity during the "nervous generation" of the interwar years. His mentality was formed at a time when many of the traditional certainties of American thought and culture were being undermined by totalitarianism and war and by recent developments in philosophy, science, and technology. Miller's writings register these trends and the crisis they produced. But Miller did not despair or lapse into a stoical acceptance of human impotence in the manner of Joseph Wood Krutch and *The Modern Temper*. Rather, he looked upon the dissolution of traditional absolutes, for all of the insecurity that disintegration caused, as providing an opportunity to call man to what he felt was man's true, Promethean destiny. In the final analysis, Miller was a moralist who preached that life took on meaning in the act of what might be called existential rebellion. Man lives in a mysterious and potentially dangerous universe, but man's responsibility is to face this environment without flinching, to defy the wilderness about him,

⁸¹ Miller, *Nature's Nation*, 12.

⁸² Miller, "Experiences of an American Professor at a Dutch University," *Netherland-America University League Bulletin*, 3 (1951): 5; and Higham, *Writing American History*, 64. Also see Miller, *Nature's Nation*, 289.

penetrate its mysteries, subject it to his will, and cultivate it. Although man can never succeed in emptying the universe of its mystery, he can accomplish much; the true meaning of life is derived from the defiant struggle to transcend his own ignorance and weakness. The theme of rebellion is most prominent in Miller's comments on Herman Melville:

It is not altogether fanciful to interpret the greatest of American novels, MELVILLE'S *Moby-Dick*, as in part a parable of the predicament of man in the American world. All things are pasteboard masks, his hero cries, behind which lurks a power, inscrutable power, possibly malignant power; but the free man, the American whaling captain, the man standing by himself, will strike through those masks and confront that power naked. And if it prove that the only worship which that power will respect or accept is defiance, then there is that in Captain AHAB of NANTUCKET which will wage the unequal fight.

If men were to recognize and accept the moral necessity of existential rebellion, they would be "able to resist, and will resist, the paralyzing effects upon the intellect of the looming nihilism" to which so many in modern society succumb. This is the message of Miller's secular sermon on "The Responsibility of Mind in a Civilization of Machines."⁸³

Miller's writings on the Puritans clearly exemplify his grand theme of the development of the modern mind. The later seventeenth century marked a watershed in this movement, and the New England Puritans participated in it. To him the mentality emerging in New England during this period was a positive advance over the medievalism of the founders. Despite all of its evasions, Puritan rationalism signaled modern man's growing tendency to take control of his own destiny, to run his own errands and not God's. The doctrine of preparation for salvation was one symptom of this change, and Miller's attitude toward its implications was positive. With it the federal theologians, by a tortuous and unintended route, had come to place "natural man . . . well on his way to a freedom that would no longer need to be controlled by the strenuous ideals of supernatural sanctification and gracious enlightenment, but would find adequate regulation in the ethics of reason and the code of civic virtues." Miller's great theme is especially apparent in the closing pages of the first volume of *The New England Mind*. As he pondered the prospects before his federal Puritans, he could barely contain his excitement. These men were activists, and so, for all of their medieval qualities, they were also imbued with a "great and revolutionary idea, that no force but the will of man can bring order out of the chaos of human depravity, though they did not always appreciate exactly how great or revolutionary it was." Thus, as perceived by Miller, the New England Puritans "were swinging free in time and space, masters of their own destiny, their fate in their own hands to make or mar at will."⁸⁴ These words contain the germ of Miller's conception of the meaning of America.

When placed within the broader context of Miller's scheme of historical

⁸³ Miller, *Society and Literature in America*, Leyden University Lectures of 1949 (Folcroft, Pa., 1969), 13, and *Responsibility of Mind*, 211. Also see Miller, *Nature's Nation*, 196. For further discussion of the theme of rebellion in Miller's writings, see Butts, "Perry Miller and the Ordeal of American Freedom," 393-466.

⁸⁴ Miller, *Nature's Nation*, 76-77, and *The New England Mind: The Seventeenth Century*, 449, 484.

development, the theme of the second volume of *The New England Mind* does not appear to be negative. The book was not designed merely to chronicle the decline and fall of the Bible Commonwealth. That was but one aspect of it. Like all of Miller's history, its underlying subject concerns the transformation and development of the American identity, which was part of the growth of the modern mind. Miller relished depicting crises in men's identities provoked by social and intellectual change. He was especially fascinated by divided souls—by men who feared and resisted change, yet somehow screwed up their courage, accepted change, and took the first halting steps toward establishing a new self-conception. This is the fundamental theme of the second volume of *The New England Mind*, and it is positive.

Cotton Mather is the central figure in the latter portion of that book because in him Miller saw the archetype of man's ambivalence over change. Mather had his feet in both the past and the future; he steadfastly remained committed to the founders' ideals, yet he was prepared to exploit the new movements of European thought in order to reinvigorate tradition. He thus ironically backed into the future.⁸⁵ Although Miller purportedly loathed Cotton Mather and did his best to denigrate him, the truth is otherwise. No doubt he found Mather's personality unpleasant. Still, he did not regard Mather with unalloyed disdain. He qualified his belief that "Mather is the most intransigent and impervious mind of his period, not to say the most nauseous human being," by stating in the next breath that Mather was also "the most sensitive and perceptive, the clearest and most resolute." Miller stressed Mather's neurotic personality, not out of malice but in order to demonstrate the pathos of Mather's predicament. In Mather's anguish and ambivalence Miller discerned "the pattern of mind imposed upon Puritanism by the ordeal of leaving the Middle Ages." Upon Mather, "as perhaps upon no other man living through what Paul Hazard calls the crisis of the European conscience, fell the burden of that transition." Miller genuinely sympathized with Mather's plight; he could not "help feeling a mixture of both admiration and pity at the efforts of the Mathers to meet this onslaught." In the end, Miller endowed Cotton Mather with heroic qualities. Confronting this crisis, he "was not the man to break, or to admit defeat."⁸⁶

REVISIONIST CRITICISM OF PERRY MILLER has not been salutary. It has been built upon a delusion. In trying to liberate themselves from his shadow, Miller's critics have been, in a sense, merely shadowboxing. They have been contesting figments of their own imaginations and not the reality of Miller's history. Much energy has

⁸⁵ Miller, *The New England Mind: From Colony to Province*, 402–06, 442. Robert Middlekauff's rendition of the story of declension contained in the "standard studies of the New England mind" and typified by Miller's work entirely misses Miller's understanding of Mather's anguished ambivalence, the irony of his account of the growth of the modern spirit, and the positive features of his portrait of Cotton Mather; Middlekauff, *The Mathers: Three Generations of Puritan Intellectuals, 1596–1728* (New York, 1971), vii–ix, 9, 214.

⁸⁶ Miller, *The New England Mind: From Colony to Province*, 476, 402, and "Solomon Stoddard," 312. For the belief that Miller held a uniformly unsympathetic view of Cotton Mather, see David Levin, "The Hazing of Cotton Mather: The Creation of a Biographical Personality," *New England Quarterly*, 36 (1963): 147–48, and

been wasted in controverting a caricature, and, unfortunately, in the process attention has been diverted from the deeper implications of Miller's preoccupation with "that magnificent but agonizing experience of what it is to be, or to try to be, an American."⁸⁷

The failure of Miller's critics is testimony to the power of preconception. Miller has been widely misunderstood because historians have been insensitive to his philosophical presuppositions. They have simply read into his work the terms of their own philosophies. It has been persistently assumed that he operated according to dichotomies in which head is set against heart, subject against object. These assumptions are mistaken. Instead, Miller brought to his understanding of Puritanism an outlook that combined an Augustinian sense of human finitude with an instrumental view of thought. Because he looked upon ideas as "the instruments though which the [Puritan] people strove to cope with a bewildering reality," his account of Puritan thinking is dramatic and developmental, not static and monolithic. His writings repeatedly focus on the struggle of men to impose some kind of conceptual order upon the "tumultuous chaos" or "buzzing factuality" about them.⁸⁸ In Miller's history, thought is at the service of man's passions, and man's passions are shaped in a social context. For this reason, no clear line separates intellect from emotion and subjective experience from external circumstance. Nor is there a simple distinction between what is secular and what is spiritual. Miller never lost sight of the social and political repercussions of religious impulse and theological doctrine.

What is surprising about the revisionists' work is that there are so few real differences between their findings and Miller's. All too often Miller has been unintentionally attacked with his own weapons. The unacknowledged consequence of revisionism has been to reinforce Miller's scholarship. Without grasping his philosophical premises, the revisionists have accepted and amplified many of his particular historical themes. It is ironic that the more Perry Miller's critics have struggled to break free from his grip, the more they have unknowingly embraced him.

"Introduction," in Cotton Mather, *Bonifacius: An ESSAY upon the GOOD*, ed. David Levin (Cambridge, Mass., 1966), ix–xi; and Morgan, Review of Miller, *The New England Mind from Colony to Province*, 294–95, and "The Puritan You Love to Hate," 31. Not all commentators have been blind to Miller's objective handling of the Mathers. Maurice Cranston noted that "Miller is not hostile towards these men"; Cranston, Review of Miller, *The New England Mind: From Colony to Province* (1953), in *Encounter*, 3 (1954): 72. Carl Bridenbaugh also observed that "no more just and dispassionate picture of Cotton Mather has ever been limned"; Bridenbaugh, Review of Miller, *The New England Mind: From Colony to Province* (1953), in the *Yale Review*, new ser., 43 (1954): 314. More recently, Gene Wise and Karen Lystra have also noted positive aspects of Miller's treatment of the Mathers; see Wise, *American Historical Explanations*, 399; and Lystra, "Perry Miller and American Puritan Studies," 280.

⁸⁷ Miller, *Nature's Nation*, 174.

⁸⁸ Miller, "Preface to the Beacon Press Edition," in his *The New England Mind: From Colony to Province*. Miller's words are reminiscent of William James's description of pure experience as being "a big blooming buzzing confusion"; James, *Some Problems of Philosophy: A Beginning of an Introduction to Philosophy* (1911; reprint edn., New York, 1948), 50.

AHR Forum

The Rise and Fall of the
Western Civilization Course

GILBERT ALLARDYCE

"THE MOST DIFFICULT QUESTION WHICH NOW CONFRONTS THE COLLEGE TEACHER in history seems, by general agreement, to be the first year of the college course."¹ So remarked Harvard historian Charles Homer Haskins in 1905 to the American Historical Association (AHA). Historians, Haskins explained, having established specialized instruction in their discipline at advanced college levels, now faced the new task of developing a course to serve the general education of freshmen and sophomores. This project, carried forward by the next generation, led to the Western Civilization course.

The rise of "Western Civ" is one of the great success stories in the history of the historical profession in America. For a time between the First World War and the campus protests of the 1960s, all roads led to the Western Civ class. Compulsory enrollment requirements at many institutions brought liberal arts students from every discipline, and from science programs and professional schools came others in mass numbers to brush with "culture" in a class renowned for grand ideas and great books. Classrooms filled, budgets bulged, teachers multiplied. In the discussion sections that became a feature of the course virtually everywhere, three generations of "teaching assistants," the best and the brightest graduate students in European history, began their apprenticeship in college instruction. Western Civ, more than any other academic invention, brought European history to power in the college curriculum, and the easy acceptance of the class in colleges across the country indicated that it represented an idea about the Old World whose time had come. By hindsight, scholars have interpreted the course as a product conditioned by the era of the two world wars, a time when Americans envisioned themselves as partners with the European democracies in a great Atlantic civilization, formed from a common history, challenged by a common enemy, and destined to a common future.² If so, Western Civ not only expressed this particular ideal of

Some of the research for this essay was supported by a grant from the School of Graduate Studies and Research, University of New Brunswick. I wish to thank my colleague Professor Ann Gorman Condon for her advice and encouragement.

¹ "Report of the Conference on the First Year of College Work in History" in *The Annual Report of the American Historical Association for 1905*, 1 (Washington, 1906): 149.

² Leonard Krieger, "European History in America," in John Higham *et al.*, *History* (Englewood Cliffs, N.J., 1965), 235–313.

America's place in history but educated youth of the period to see the world in these terms. Historians are accustomed to writing about men of influence in human affairs. When they write the history of their own influence in the present century, the Western Civilization course will have an important place in the story.

Today, the grand old course still clings to life at various schools. At many other locations, however, it passed with the passing of the 1960s. Somewhat suddenly, after a half-century of uninterrupted success, Western Civ seemed less appealing to students and less defensible to faculty. In a period of expansion, experiment, and self-expression, the venerable reputation of the course was no match for the moral force of a curricular philosophy of *laissez faire*, education without compulsion or prerequisites. When compulsion stopped, enrollment dwindled, and across the nation, one after the other, Western Civ courses were decommissioned like old battleships. Easy come, easy go.

After every revolution, however, comes Thermidor. Looking back on the old course, historians at an AHA session in 1976 returned to the problem of the introductory course. There Professor William H. McNeill of the University of Chicago explained that Western Civ originated in "a great idea about the whole human past," the idea of history as the evolution of freedom. This notion, now described quaintly as the Whig interpretation of history, once provided a vision useful and inspiring. But with time, McNeill remarked, the vision faded, the idea was no longer believed, and new material mingled with the old subject matter, clouding over the central theme. Western Civ became a jumble, a course without memory of its own origins or convictions. When challenged, historians in this proud tower gave up without a fight, retreating into the sanctuaries of their own special areas. The result, McNeill observed, was to turn history programs into a pyramid without a base, a superstructure of advanced courses without the supporting foundation of an introductory class where students could be prepared and recruited. His lament was not that historians failed to defend an outmoded concept of the past but that they had failed to develop a new one to take its place. This he found "amazing, suicidal, absurd"—"the central failure of our profession in the last two decades." Historians, it appeared, were on the road that teachers of the classics had followed to irrelevance and antiquarianism. "The only thing that can rescue us from such a fate," McNeill concluded, "is to find something worth teaching to undergraduates *en masse*: something all educated persons should know; something every active citizen ought to be familiar with in order to conduct his life well and perform his public duties effectively."³

MCNEILL'S STATEMENT IS THE WINGED LANGUAGE of the general education philosophy, the ideal of the civilizing mission of colleges to educate citizens for community life. His remarks in this connection can be recognized as part of a revival in the 1970s of concern with restoring a curriculum ravaged by protest and change. At Harvard in 1974, Dean Henry Rosovsky deplored the loss of "an older community

³ McNeill *et al.*, "Beyond Western Civilization: Rebuilding the Survey," *History Teacher*, 10 (1977): 509–15.

of beliefs and values" and called upon his faculty to "redefine our collegiate purpose."⁴ The resulting reconstruction of a core curriculum at Harvard was only the most conspicuous success in a resurgence of general education that continues across the nation. Therefore, the proper time has come to review the story of Western Civ. It was the pre-eminent course of the earlier general education movement, sharing all of the ideals and illusions of the movement itself. To study the rise and fall of Western Civ, indeed, is to study the historical background to present debates about the curriculum.

The history of the course is written here in terms of the concept of general education. "General education," explained a committee of educators in 1947, "is the term that has come to be accepted for those phases of nonspecialized and nonvocational learning which should be the common experience of all educated men and women."⁵ The movement for general education is best understood, however, in connection with its origins after 1900 as an emotional response to the perceived "chaos" of the new elective system. American higher education was then in the closing decade of a historic transformation that, within a single generation, sundered the classical curriculum of the old liberal arts colleges and installed the specialized programs of the modern university. Historians have explained how this "academic revolution" between 1870 and 1910 established the reign of the graduate school, professional disciplines, and research.⁶ Generally associated with President Charles William Eliot of Harvard, the elective system was truly the guillotine of this revolutionary process, dismembering the curriculum in the name of the equality of courses and the liberty of students to choose among them. Underlying the elective system was a philosophy of diversity, a perception that the "knowledge explosion" and the modern division of labor required men to vary their purposes and the community to divide its talents. "For the individual," Eliot advised, "concentration, and the highest development of his own particular faculty, is the only prudence. But for the State, it is variety, not uniformity, of intellectual product, which is needful."⁷ To some, the resulting system of "curricular do as you please" was the principle of liberty in education; to others, it was the law of the jungle.

One response was the general education movement. Without original thinkers or founding texts, it began in an emotion to integrate modern learning into forms comprehensible and useful to nonspecialists. A freshman without comprehension of the unity of knowledge, explained historian Preserved Smith, was not prepared to grasp the meaning of separate disciplines. "At the very outset of the college course," he urged, the student should have "set before him, in a panorama

⁴ Rosovsky, "A Letter to the Faculty on Undergraduate Education, October 1974." Harvard University Archives, Cambridge, Mass. [hereafter, HUA].

⁵ *Higher Education for American Democracy: A Report of the President's Commission on Higher Education* (New York, 1947), 49. See further Lewis B. Mayhew, "General Education: A Definition," in Mayhew, ed., *General Education: An Account and Appraisal* (New York, 1960), 1–24. The history of general education remains to be written. Useful is Russell Thomas, *The Search for a Common Learning: General Education, 1800–1960* (New York, 1962).

⁶ Laurence R. Veysey, *The Emergence of the American University* (Chicago, 1965); and Richard Hofstadter, "The Revolution in Higher Education," in Arthur M. Schlesinger and Morton White, eds., *Paths of American Thought* (Boston, 1963), 269–90.

⁷ Eliot, *A Turning Point in Higher Education: The Inaugural Address of Charles William Eliot as President of Harvard College* (Cambridge, Mass., 1969), 10.

infinitely reduced in scale but true to proportion, the whole scheme of things entire as we know them.”⁸ The community of scholars in America, such men believed, must be a democratic community, citizens educated as undergraduates in shared values that made possible an enlightened public consensus. General education, therefore, was a philosophy of unity: unity of knowledge, unity of the curriculum, unity of education and life. Thus is revealed its historical significance as a defensive reaction arising at the close of the academic revolution against the forces that threatened to engulf higher education: specialization, professionalism, and vocational training. These things taught individuals to be different; general education taught them what they should know in common.

There is an old saw to the effect that changing a curriculum is like trying to move a graveyard. On its own, the general education movement was not up to the task. Specialization was the prevailing tide in higher education; general education merely slackened the flow. Even this limited success, however, depended on influences outside of the movement itself. In particular, the history of general education is inseparable from the period of the world wars, when, for a time, a sense of common purpose caused the language of the movement to strike a responsive chord in the nation. Otherwise, life was difficult. If the ideals of general education—nonspecialized learning, core course requirements, and the interdisciplinary approach—sustained the movement as a fighting force, they also placed it in opposition to dominant trends within the university. At most schools, therefore, the program found a place to grow only in unclaimed areas away from the empires of academic departments.

Thus developed the movement's preoccupation with the freshman and sophomore years. In this no man's land between high school and advanced college work at the junior-senior level, the Western Civ class emerged as the ancestral course of the general education curriculum. This article recounts the history of the course in connection with the four institutions—Harvard, Chicago, Amherst, and Stanford—represented at the AHA session in 1976, at which Professor McNeill called for rebuilding freshman history on the general education ideal, and a fifth institution, Columbia, where Western Civ purportedly was “invented” in 1919. Admittedly, the number of schools in this sampling is too limited. Western Civ classes appeared across the country in immense variety and rich contrast. What is presented here is a view from the commanding heights. The universities of Chicago, Columbia, and Harvard dominated the development of general education in the United States, and each has an official history of its own pioneering role in the movement.⁹ Amherst and Stanford, by extension, provide contrasting studies of schools directly influenced by these three great universities. What follows, then, is a selective history of the Western Civ course, piecing together, in the form of a mosaic, the experiences of these five institutions.

⁸ Smith, “The Unity of Knowledge and the Curriculum,” *Educational Review*, 45 (1913): 340.

⁹ See *The Idea and Practice of General Education: An Account of the College of the University of Chicago* (Chicago, 1950); *A College Program in Action: A Review of the Working Principles at Columbia College* (New York, 1946); and the Harvard report, *General Education in a Free Society* (Cambridge, Mass., 1945).

ORIGINS OF INTRODUCTORY EUROPEAN HISTORY: HARVARD, 1873–1904. The Western Civ course was a characteristically American invention. The United States inherited a Western perception of civilization as a process that began in the ancient Near East, evolved through classical Greece and Rome, and was transmitted to medieval and modern Europe. European “civilization,” in this way, was the sum of world history. In Europe, historians during the nineteenth century divided this universal history into separate national strands. Nation-states became the interest of historians. While national history thus came to dominate schools on the Continent, however, European history in America commonly continued to be presented *en bloc*. As a result, scholars have noted, more European history was taught in the United States than in Europe itself. Leonard Krieger has explained how this continuing perception of Europe as a unit gave the writing of European history in the United States a bent for broad interpretation, a concern with common experiences underlying national differences. What was stressed, therefore, was not political flux but the enduring achievements of the mother culture. What made Europe a “civilization,” remarked John Franklin Jameson in 1890, was not politics but culture; in thought and letters, he explained, American historians would discover “abundant evidence of a common European life.”¹⁰

The organizing ideas for Western Civ were, therefore, long present in historical consciousness. What gave them new meaning in the early twentieth century, scholars have concluded, was the emergence of the United States from isolation to partnership with Europe in a wider world. But Western Civ did not come into existence fully assembled, nor was it conceived in one swoop at Columbia in 1919. Rather, the course and the concept came together piece by piece, not by grand design but as a makeshift response to the effects of the academic revolution on the history curriculum. Stated briefly, specialization and the elective system created, by reflex, the need for a general course.

Beginning the story with Harvard is instructive, because Harvard was the hothouse of the elective system and thus the place where the special problems of the introductory course in history emerged most clearly. The subject of history had no place in the classic liberal arts and only gradually did it intrude—beginning at the junior-senior level—upon the curriculum. When President Eliot was inaugurated in 1869, for example, all history at Harvard was taught by a “dear old gentleman” without training in the discipline. But the elective system was the undoing of his kind. Out went the amateurs and in came the experts: more courses, more fields—and more confusion. In describing the curriculum of 1884, one professor admitted that junior-senior offerings in history were expanding without order or direction, with everything in “more or less confusion.”¹¹ Out of this disorder was born the

¹⁰ Fritz Stern, “Europe’s Past and America’s Experience,” in Joseph S. Roucek, ed., *The Teaching of History* (New York, 1967), 27; Krieger, “European History in America”; and Jameson, “The Development of Modern European Historiography,” *Atlantic Monthly*, 66 (1890): 322.

¹¹ Henry E. Scott, “The Course of Study in History, Roman Law, and Political Economy at Harvard University,” in A. D. White, ed., *Methods of Teaching History* (Boston, 1886), 170. On Harvard’s “dear old gentleman,” Henry W. Torrey, see Samuel Eliot Morison, *Three Centuries of Harvard* (Cambridge, Mass., 1936), 293, 347.

introductory course. As early as 1873 a course for sophomores, "Outlines of General History," was developed out of earlier reading requirements on classical antiquity. In 1880 it became "History 1: Medieval and Modern History," a course with a long life to come. Opened to freshmen in 1884, it was established by 1890, in a regulation that reversed the logic of the elective system, as a prerequisite to advanced classes in history.¹² Specialized instruction at one end of the curriculum had made necessary general instruction at the other, a class for freshmen and sophomores that was comprehensive and preparatory.

Such classes, however, had problems of their own. Eliot reported in 1880 that old, hide-bound methods of student recitation had "well-nigh disappeared" at Harvard and that lectures and discussion were in fashion. Remarked a commencement speaker in 1886, "This is one of the greatest educational discoveries of modern times—that the business of a teacher is to teach." But, as the lecturer came to head the class, the student disappeared into the audience. The rise of large introductory courses increased the distance between them. In 1882 only five classes at Harvard enrolled as many as one hundred students; by 1901 fourteen classes each had over two hundred. In the following year the university's Committee on Improving Instruction sounded the alarm: "there is in the college today," it concluded, "too much teaching and too little studying."¹³ Large lecture courses concealed students from faculty surveillance, made standards unenforceable, and encouraged traffic in lecture notes. Such large classes, the committee reported, were a practical necessity, but so were new arrangements to bring students out of the crowd and make them speak again.

History 1 was a case in point. When Professor Archibald Cary Coolidge took over the course in 1894, the enrollment was 364 and rising. "Three times a week," wrote Coolidge's biographers, "he lectured to mobs of freshmen and sophomores." But, as the mobs grew larger, the results of the lecture system became more uncertain. In 1896 a number of graduate student "assistants" were delegated to inspect lecture notes and to quiz students individually outside of class. This so-called check-up system, however, was no favorite with students. Finally, in his last year with the course in 1903–04, Coolidge restructured History 1, reducing lectures to two per week and incorporating the "section method," an experiment (already operating in other Harvard courses) with subdividing large classes into smaller groups for weekly meetings with graduate assistants.¹⁴ So was developed what became known in early debates among historians on the introductory course in history as the "Harvard method": lectures to the mass by a faculty member and sections in the care of young apprentices.

The lecturer was the great man of the enterprise. Originally a mode to transmit ancient learning, the lecture method had been revitalized in German universities as

¹² Unless otherwise noted, references in this article to course descriptions, regulations, and enrollments are drawn from annual school catalogues and reports.

¹³ James Freeman Clarke, Commencement Address, as quoted in Morison, *Three Centuries of Harvard*, 347; and "Report of the Committee on Improving Instruction in Harvard University" (1902), 4, HUA.

¹⁴ Harold J. Coolidge and Robert H. Lord, *Archibald Cary Coolidge: Life and Letters* (Boston, 1932), 41; and Ephraim Emerton and Samuel E. Morison, "History, 1838–1929," in Morison, ed., *The Development of Harvard University since the Inauguration of President Eliot, 1869–1929* (Cambridge, Mass., 1930), 170–71.

a means to communicate new knowledge derived from original research. A crowded classroom of American freshmen, however, was no place for learned lectures. Instruction here had to be both education and art. The report of the Committee on Improving Instruction explained in 1902,

The larger these courses grow, the more evident it becomes that the object of the lectures in them is not so much to impart concrete information as to stimulate thought and interest in the subject; and since the stimulus depends in part on the attitude in which the audience stands towards the lecturer, it is important that these courses should be conducted by the men who have already achieved a reputation. Indeed . . . to be effective the lecture course must be conducted by the best lecturers in the university.

In the Harvard system, therefore, the development of the discussion section did not so much diminish the lecturer as exalt him—and provide a staff of helots to do the work. “The function of the instructor is to stimulate and interest his hearers,” proposed the committee report, “while the responsibility for seeing that the work is done, for helping and explaining, and for maintaining the standard of the course, must rest chiefly with the assistants, who come into more immediate contact with the students.”¹⁵

Having formed discussion sections, Coolidge had to give them a purpose. To him, freshman history was factual history, and quizzes, map drills, and recitations on lectures and textbooks were the order of the day in section work.¹⁶ Elsewhere, however, this emphasis on memorizing detail was giving way to efforts to promote understanding through readings from original sources in English translation. Historians first debated the value of this “source method” at a meeting of the AHA in 1897; in affirming the prevailing conception of history as a body of knowledge to be communicated to undergraduates by lectures and textbooks, members approved a limited and subordinate use of primary materials to “vitalize” events and promote critical debate. This view, carried into the high schools, became identified with a democratic education. The Committee of Seven, for example, appointed by the AHA to recommend a history program for secondary schools, praised the German contribution to historical studies but observed that German gymnasium methods of rote learning were “not the system for making American citizens.” Habits of discussion, the committee concluded in 1899, were essential to a system of individual initiative.¹⁷ When, therefore, committee member Charles Homer Haskins succeeded Coolidge as instructor of History 1 in 1904, the source method was introduced into the discussion sections. Thus was completed the form of the introductory course at Harvard. The content took more time.

The Committee of Seven was the first group of professional historians to review systematically the condition of history in the high schools. The members made no secret of their ambition to drive out the old, overstuffed course in “General History,” a one-year, headlong survey of “world” history from ancient civilization to

¹⁵ “Report of the Committee on Improving Instruction,” 7–8.

¹⁶ See his remarks to the AHA in 1898, “The New Haven Meeting of the American Historical Association,” *AHR*, 4 (1898–99): 413.

¹⁷ *Annual Report of the American Historical Association for 1897* (Washington, 1898), 5; and *The Study of History in Schools: Report to the American Historical Association by the Committee of Seven* (New York, 1899), 11–13.

modern Europe. To replace it, they recommended a sequence of four courses: first, in grade nine, ancient history, followed in grade sequence by European history, English history, and American history. The logic of this "block system" was simply to extend instruction through high school and to place American history in European perspective. "American history," remarked a committee member, "is in the air—a balloon sailing in mid-heaven—unless it is anchored to European history."¹⁸ Although the program was adopted widely in the schools, many students, it was noticed, made a habit of leapfrogging the "block" on European history. Reports indicated that pupils completed ancient history (at that time, a common requirement for college entrance), switched to other subjects in the following two grades, and returned in their last year to take American history.¹⁹ The effect was to strengthen the conviction of those college instructors who, in discussions on the introductory course, supported European history as the proper subject for freshmen. "Now the real reason why we introduced into American colleges this general course on European history," commented Charles Homer Haskins in 1906, "is because students did not bring it to college with them."²⁰

Haskins was a prominent personality in the advance of this version of the introductory course. He sat as chairman at various conferences on freshman history, advocated the cause in the AHA, and promoted his course at Harvard as a model in the field. Focused on European history, combining lectures and discussion sections, textbooks and primary sources, History 1 in the early years of this century no doubt had an important place in debates on the freshman survey. But Haskins led it backward, not forward, in time. Ancient and American history dominated the high schools; medieval history, Haskins believed, would educate freshmen to connect one world with the other. "The year devoted to the Middle Ages," he proposed, "bridges the gap between ancient and modern studies, but by showing the remote origin of modern institutions and culture it deepens the sense of indebtedness to the past and furnishes something of the background so much needed in our American life."²¹ In 1904, History 1 became "Medieval European History."

Elsewhere, the trend was in the other direction. Rising with the "New History" (about which I will say more later) after the turn of the century, an interest in modern history spread through colleges and high schools.²² At Harvard itself, enrollment in History 1 fell from 380 in 1904 to 250 in 1911. The following year, Haskins reintroduced modern history to the course, and History 1 became "European History from the Fall of the Roman Empire to the Present Time." The Harvard course was a descendant of the academic revolution, formed through the

¹⁸ Lucy M. Salmon, as quoted in *The Study of History in Schools*, 194.

¹⁹ "Problems of the Freshman Course," *History Teacher's Magazine* [hereafter, *HTM*], 1 (1910): 152.

²⁰ "Report of the Conference on History in the College Curriculum," in the *Annual Report of the American Historical Association for 1906*, 1 (Washington, 1907): 122.

²¹ Haskins, "The Introductory Course in History," *HTM*, 1 (1910): 95.

²² Increased demand for modern European history was noted by the Committee of Five, appointed by the AHA in 1907 to review the recommendations of the Committee of Seven: *The Study of History in the Secondary Schools: Report to the American Historical Association by a Committee of Five* (New York, 1912), 53–57. Also see the college survey in Raynor W. Kelsey, "Recent Changes in the Teaching of History in the Colleges and Universities of the Middle States and Maryland," *HTM*, 6 (1915): 207–10.

efforts of historians to adjust their methods of instruction to the special features of the freshman year. Some scholars have interpreted the result as the prototype of the Western Civ class and recognize Archibald Cary Coolidge as the father of the course.²³ More accurately, however, History 1 was a point of departure. At the start of the twentieth century, educational thought was already leaving it behind.

THE MAKING OF WESTERN CIV: COLUMBIA, 1905–29. The years after 1905 were a period of experimentation in the introductory history class. No other course, one historian observed, matched the pace of innovation in freshman history.²⁴ Significantly, these changes took place in an intellectual environment different from that in which developed the introductory course at Harvard. The counterrevolution was coming in American education, the movement to restore unity to the undergraduate curriculum. If, to this point, the introductory course in European history was conditioned by the academic revolution, thereafter it was conditioned by the reaction of general education.

Columbia led the way. Few schools were more deeply under the German spell of specialization; few broke out of it with greater vengeance. That shift, that break is epitomized in the career of Columbia's President Nicholas Murray Butler. He began as a paladin of professional scholarship, critical of the "idling and dawdling" of undergraduate work and committed to the hegemony of the graduate school. Then, after 1900, he followed the journey of educational thought. It led him first to those seeking to recover the "inner unity" of knowledge and finally, under the patriotic influence of the First World War, to the cause of a common education for citizenship. "The more men and women who are being trained up to twenty to twenty-one years of age without any reference whatever to a particular vocation or occupation," Butler told the Columbia trustees in 1918, "the better for the citizenship, the intelligence, and the moral and spiritual life of the nation."²⁵

Such was the background to the creation at Columbia in the following year of the freshman class in "Contemporary Civilization," mother of Western Civ and—according to Columbia sources—the progenitor of the general education tradition.²⁶ This course itself was, however, successor to classes leading back to an early version of introductory history at Columbia: History A, "Epochs of Ancient,

²³ Robert F. Byrnes, "Archibald Cary Coolidge: A Founder of Russian Studies in the United States," *Slavic Review*, 37 (1978): 652. For the suggestion that Western Civ was pioneered by H. Morse Stephens at Berkeley in 1911, see Laurence Veysey, "Stability and Experiment in the American Undergraduate Curriculum," in Carl Kaysen, ed., *Content and Context: Essays on College Education* (New York, 1973), 51. In comments to the AHA in 1905, however, Stephens indicated that he was influenced by the Harvard system; "Report of the Conference on the First Year of College Work in History," 172–73. For a description of his course at Berkeley, see Everett S. Brown, "Freshman History at the University of California," *HTM*, 7 (1916): 268–69.

²⁴ Lucy M. Salmon, "Vassar College Introductory Course," *HTM*, 1 (1910): 145. After first discussing freshman history in 1901, the AHA took up the subject again at meetings in 1905 and 1906. The ferment among instructors is revealed in the early volumes of the *History Teacher's Magazine*, founded in 1909 to increase communication between high school and college teachers.

²⁵ *Annual Report of the President and Treasurer to the Trustees of Columbia University, 1918* (New York, 1919), 12. On the vogue of the "unity of knowledge" after 1900, see George Haines and Frederick A. Jackson, "A Neglected Landmark in the History of Ideas," *Mississippi Valley Historical Review*, 34 (1947–48): 201–20.

²⁶ Justus Buchler, "Reconstruction in the Liberal Arts," in Dwight C. Miner, ed., *A History of Columbia University on Morningside* (New York, 1954), 49.

Medieval, and Modern History." By 1900, this troubled sophomore course in old-style "general" history was taught in large lecture classes by two young instructors from the bottom rank of the Department of History. At a time when the AHA was trying to root out world history from the high schools, Columbia implanted it as an undergraduate requirement. The result, critics complained, was an overburdened, chaotic, and unteachable course.²⁷

The man who brought order out of this chaos was James Harvey Robinson. Robinson never taught History A (although he was assigned to simplify its content) nor any other introductory course, but no one did more to shape the first-year history course in American colleges. Scholars remember him for his promotion of the "New History," a history useful and "progressive," united with the social sciences and brought to the service of social reform. Just as important was his promotion of a simplified, thematic, interpretive version of European history for high schools and colleges, a general history free of "every unessential detail which serves only to obscure the great issues and transformations of the past."²⁸ Critics have observed that Robinson was not an original thinker, that he was merely "representative" of intellectual trends, a propagator of ideas already current among historians. True enough. But no one represented them better or propagated them more effectively. At the cost of a career in original scholarship, he committed himself to the cause of this nonspecialized undergraduate history, and to all of the committee work, polemics, and textbook writing that went with advancing it within the educational establishment.

After the publication of Robinson's first textbook in 1903, recalled an admirer, no one could return to writing introductory history in the old style of past politics.²⁹ The crucible of ideas for Robinson's successful line of undergraduate and high school texts was his popular graduate class at Columbia, "History of the Intellectual Class in Europe." Where instructors at Harvard began the introductory course as general history and reduced it over time to the more manageable unit of the medieval period, Robinson began his class as medieval history and enlarged it to general history. His students have explained how he started with readings from medieval documents, extended coverage to the French Revolution, then continued year by year to reach further into the past for the origins of European ideas and further into the present for their consequences. As Harry Elmer Barnes reminisced,

The Middle Ages sent him back to pagan culture, pagan culture to the ancient Orient, oriental culture to the mental life of primitive man. . . . Similarly, his interest in science and criticism led him to go forward from the French Revolution to a study of the growth

²⁷ See, for example, the criticisms of Charles Homer Haskins, "The Historical Curriculum in Colleges," in *Minutes of the Second Annual Convention of the Association of History Teachers of the Middle States and Maryland, March 11-12, 1904*, 17.

²⁸ Robinson, "Sacred and Profane History," in the *Annual Report of the American Historical Association for 1899*, 1 (Washington, 1900): 534-35.

²⁹ J. Salwyn Schapiro, "James Harvey Robinson," *Journal of Social Philosophy*, 1 (1936): 281. For a comparison of Robinson's works with previous texts in the field, see Luther V. Hendricks, *James Harvey Robinson, Teacher of History* (New York, 1946), 68-89.

of modern science and its influence upon material culture and the world of ideas. The course thus became more universal in its scope and more interpretive in character.³⁰

By 1915 the class syllabus forecast the outline of the Contemporary Civilization course to come.³¹

Others had judged such general history to be too vast and disorderly. Robinson made it manageable by making it “modern.” Like most American historians of Europe at that time, Robinson had been trained in medieval history, and his appointment at Columbia in 1895 came in recognition of his promise in that field. Somewhere along the line, however, he turned modernist with a passion. In debate with Charles Homer Haskins at the AHA in 1907, Robinson argued that medieval history had been overstressed in college teaching and that coverage of the Middle Ages should be diminished, simplified, and subordinated to that of modern history. Robinson became fond of a particular question about the contemporary world: “How did we get this way?” The answer, he believed, had to come from “living history,” that part of the past that continues in the present. The result, in his textbooks, was history presented on the model of evolution in the biological sciences, a central narrative that excluded everything defunct, anomalous, and sensational, that eschewed all of the lost causes and dead ends. Recalled Robinson,

Only gradually did the writer come to conceive of history as something far more vital than the record of bygone events and the description of extinct institutions. He then saw that if history was to fulfill its chief function and become an essential explanation of how our own civilization came to take the form it has, and present the problems that it does, a fresh selection from the records of the past would have to be made. Much that had been included in historical manuals would of necessity be left out as irrelevant or unimportant. Only those considerations would properly find a place which clearly served to forward the main purpose of seeing more and more distinctly how this, our present Western civilization, in which we have been born and are now immersed, has come about.³²

For most citizens, Robinson proposed, this was the only history worth knowing. Actually, the whole project was rather unhistorical, Whiggish, and Eurocentric. In substance, Robinson’s textbooks are basically intellectual history (with an overlay of economic, social, and cultural developments), the story of what he called “the mind in the making,” the perceived evolution of rationalism, science, and liberal values. In form, the narrative begins in a tight description of progress from prehistory to the close of the Middle Ages, then broadens in the coverage of modern Europe and its influence on other continents. In effect, the past is subordinated to the present, recent history becomes “relevant” history, the human past becomes the prologue to European history, and Europe is interpreted as the seat of modernity, the source of

³⁰ Barnes, “James Harvey Robinson,” in Howard W. Odum, ed., *American Masters of Social Science* (New York, 1927), 375.

³¹ Compare Robinson, *An Outline of the History of the Intellectual Class in Western Europe* (New York, 1915), and the Contemporary Civ staff publication, *Introduction to Contemporary Civilization: Syllabus, 1920* (New York, 1920).

³² “Report on the Conference on Medieval European History,” in the *Annual Report of the American Historical Association for 1907*, 1 (Washington, 1908): 67; and Robinson, *The Ordeal of Civilization* (New York, 1926), 4.

“contemporary ways of doing and thinking.” Western history in Robinson’s texts is, therefore, “high history,” overarching the past of other peoples. Westerners, of course, had long universalized European history into the general history of mankind. More effectively than others of his generation, Robinson made this old general history into a “modern” history of Western civilization.

So, before the First World War, American educators were prepared intellectually for the coming of the Western Civ course. What prepared them emotionally was the war itself. During the Great Crusade, historical perceptions of a pioneer America, formed by the frontier experience, gave way to an alternative vision of the nation’s connection with Europe. The war, in this sense, vitalized an interpretation of history that gives the United States a common development with England and Western Europe and identifies this “civilization” with the advance of liberty and culture. Scholars have described how a sense of patriotic purpose swept campuses, inspiring academics to bring education to the service of the nation. Many historians now felt the lure of a “useful” history geared to modern events in Europe.³³ And many in other fields felt a duty to transcend disciplines in order to create a general education for citizenship. Those at Columbia later remembered the Contemporary Civilization course of 1919 as a “war baby,” born of the struggle to make the world safe for democracy.³⁴ More precisely, it was the child of a strange marriage between war propaganda and the liberal arts: the “War Issues Course” of the Students Army Training Corps (SATC).

The War Issues course was part of an extraordinary episode in American higher education. In 1918, as military conscription threatened to decimate college enrollments and budgets, school officials came together with the War Department to create SATC, units of conscripts to be drilled and quartered on college campuses as military officers in training. In October, at over five hundred institutions, the majority of male students, more than one hundred and twenty-five thousand young men in military uniform and under military discipline, commenced training programs apart from the regular curriculum. American colleges became military camps and professors mixed with drill instructors in the education of student soldiers. Compulsory for all was the War Issues course, a class designed to explain the origins and meaning of the European conflict. Explained the national course director from Washington, “This is a war of ideas, and . . . the course should . . . give to the members of the Corps some understanding of the view of life and of society which they are called upon to defend and of that view against which we are fighting.”³⁵ This required the integration of material from a variety of intellectual disciplines, and the War Department called upon each college to assign to the planning and teaching of the course the best men available from the departments

³³ On the effect of the war in promoting modern European history, see Samuel B. Harding, “What the War Should Do for Our History Methods,” *Historical Outlook* [hereafter, *HO*], 10 (1919): 188–90. Also see Carol S. Gruber, *Mars and Minerva: World War I and the Uses of the Higher Learning in America* (Baton Rouge, 1975).

³⁴ Harry J. Carman, “An Example of an Integrated Program in the Social Sciences,” in B. Lamar Johnson, ed., *What about Survey Courses?* (New York, 1937), 249.

³⁵ Frank Aydelotte, *Final Report of the War Issues Course of the Students Army Training Corps* (Washington, 1919), 42–46. On SATC, see C. R. Dooley, *Final Report of the National Army Training Detachments* (Washington, 1919).

of history, government, philosophy, economics, and literature. Here, critics warned, tumult was to be expected. Instead, academic rivalries dissolved in the heat of patriotism. As a result, therefore, the War Issues course was a successful experiment in general education by government decree: compulsory, interdisciplinary, an exercise in common learning for the duties of citizenship. Many educators were relieved to see it end in December 1918; other recognized it as an idea of instruction whose time had come.

From the War Issues course came the various Citizenship classes that appeared at colleges after the war. When the course ended at Columbia, some of the instructors involved appealed immediately for a "peace issues" course to take its place. Wrote a faculty dean,

It is not surprising . . . that those who have had to do with this course are beginning to ask themselves if it does not constitute the elements of a liberal education for the youth of today. Born of the consciousness that a democracy needs to know what it is fighting for, it has awakened a consciousness of what we, as a people, need to know if our part in the world of today is to be intelligent, sympathetic, and liberal. In the past, education was liberalized by means of the classical tradition. . . . If education is to be liberalized again, if our youth are to be freed from a confusion of ideas and standards, no other means looks so attractive as a common knowledge of what the present world of human affairs really is.³⁶

James Harvey Robinson, preparing to leave Columbia, was not involved in the discussions of 1918–19 that led from the War Issues course to the Contemporary Civ class. But his ideas remained to give depth and substance to a class founded on the simple world view of Allied war propaganda. Early statements about the course, which today appear flatfooted in their chauvinist idealism, reveal the political purpose of Contemporary Civ to promote liberal opinion, acculturate the young, and create "a citizen who shall be safe for democracy."³⁷ The achievement of those who planned the syllabus was to subsume these political designs within the organizing idea of Robinson's history: the view of the past as the progress of reason and liberty. Without this sustaining historical framework, one may speculate, the Contemporary Civilization course would likely have faded away with the other citizenship classes of the postwar years, victims of peacetime moods and the debunking of the wartime patriotism of the academic community.

Just as important as the power of Robinson's history as an organizing idea, however, was its power as an integrating discipline. The interdisciplinary sweep of the New History contributed to Contemporary Civ a mode of incorporating other fields of knowledge into a panoramic overview of the process of civilization. The course was designed in committee by representatives from the departments of history, economics, philosophy, and government, and it was taught from a common syllabus by a staff drawn from all of the departments involved. Its technique of instruction was the small discussion class, and a successful class was judged to be one

³⁶ Frederick J. E. Woodbridge, "The 'Issues of the War' Course in the SATC Schedule," *Columbia Alumni News*, 10 (1918): 217. Woodbridge, dean of the graduate faculty, directed the course at Columbia.

³⁷ The phrase is that of Columbia Dean H. E. Hawkes; see his "A College Course on Peace Issues," *Educational Review*, 58 (1919): 150.

in which the students were unable to detect the department of their teacher. In a word, integration was the Contemporary Civ ideal. This, in part, explains the appeal of the class to many faculty and administrators as a form of common learning, a uniform orientation course for the mass of freshmen who entered the college after the military demobilization of 1918–19. Commented President Butler to the trustees, “A result of prescribing this course for all freshmen is to make sure that every student in Columbia College has a common starting point and a single point of vantage from which to study, to understand and to appreciate the world of nature and of men.”³⁸

Although Contemporary Civ was approved by only a “bare majority” of faculty in 1919, the opponents of the class were soon scattered. Butler in 1922 declared the course a “pronounced success” and in 1924 polls among graduating seniors began to identify it as the most valuable class in the college.³⁹ By 1929, when the faculty voted to extend Contemporary Civ to a two-year course, opposition was nowhere to be found. Thus, a compulsory two-year requirement in Contemporary Civilization was established: the freshman year (called CCA) on the historical-cultural background to the modern world, the sophomore year (called CCB) on the problems—from a social science perspective—of contemporary American society. Thereafter, plans were advanced to found other general education classes, to spread the spirit of the new course through the college, and to adjust advanced offerings to the work of the freshman year. General education had won a battle at Columbia—and the war spread elsewhere. With the success of Contemporary Civ, recounted a later Columbia report, there began “a quiet and gradual revolution in undergraduate instruction throughout the United States.”⁴⁰

Stated differently, general education began its advance in American colleges on ground prepared by the New History. The intelligent citizen, many educators had come to believe, was historically minded. When Arthur M. Schlesinger surveyed the history programs at various colleges in 1919–20, he found that the European war had heightened the appeal of history and inspired instructors of introductory courses, faced with surging enrollments, to become less interested in producing fledgling historians than in educating freshmen to be “intelligent citizens of the republic and the world.”⁴¹ This was the environment that formed the Western Civ courses. The progress of the course at this period must be seen in connection with the general progress of European history in America. Between the world wars, the modern European history curriculum first took shape in the United States. It was, as Leonard Krieger has described, European history with American relevance—liberal, “progressive,” concentrated on modern history and based on the premise of a common history that bound together the North Atlantic nations, connected the United States to the European past, and established Western preponderance in the world. Wrote Carleton J. H. Hayes, Robinson’s student and successor at Columbia,

³⁸ Columbia University, *Annual Report of the President . . .*, 1920, 22.

³⁹ On the success of the course, see Harry J. Carman, “The Columbia Course in Contemporary Civilization,” *Columbia Alumni News*, 17 (1925): 143–44. Carman, a Robinson student, early staff member in the course, and later the dean of Columbia College (1943–50), shepherded the course from its first syllabus to its heyday after World War II.

⁴⁰ *A College Program in Action*, 22.

⁴¹ Schlesinger, “The History Situation in Colleges and Universities,” *HO*, 11 (1920): 103–06.

in his popular Western Civ textbook in 1932, "For two thousand years and more, Europe has been the seat of that continuous high civilization that we call 'Western'—which has come to be the distinctive civilization of the American continents as well as of Europe, and which has latterly affected the peculiar civilizations of the 'east' more than those have affected the 'west.'"⁴²

What started as a new direction in European studies soon became an "establishment." Publications increased and programs prospered. In 1929 the *Journal of Modern History* appeared as the periodical of the growing field of European history since the Middle Ages. In 1934 the report of the Commission on the Social Sciences of the American Historical Association, written by Robinson's colleague in the New History Charles A. Beard, recommended more European history in secondary schools:

In all departments—intellectual, esthetic, and ethical—the civilization of the United States has always been a part of European, or "Western," civilization. . . . The program of social science instruction should provide for a . . . more detailed study of the evolution of Western civilization, emphasis being placed on changing modes of production and distribution, on the succession of social systems, ways of life and ethical conceptions, on the development of democratic ideals and practices, on the accumulation and spread of knowledge and learning, on the advance of science, technology, and invention, on the abiding traditions of the unity of Western culture and its growing integration in world culture.⁴³

Thus was established a support system that sustained Western Civ courses for nearly a half-century and promoted European history as the integrating discipline of general education programs.

THE CHALLENGE OF OTHER COURSES: CHICAGO, 1929–50. Survey courses, citizenship courses, great books courses, critical thinking courses—such were the names given to the experimental classes that proliferated in the 1920s. Some authorities estimate that the number of these early strivings in general education increased from two courses in 1911 to eighty-two by 1925.⁴⁴ An educational philosophy was in search of proper subject matter. Many educators believed that Contemporary Civilization at Columbia set the example. When the American Association of University Professors evaluated new courses across the country in 1922, however, it recommended instead classes on "critical thinking." For freshmen, the association report concluded, systems of logic, not the historical process, was the proper foundation of a general education.⁴⁵

No major school went further in this direction than the University of Chicago.

⁴² Krieger, "European History in America," 255–87; and Hayes, *A Political and Cultural History of Modern Europe*, 1 (New York, 1932): vii.

⁴³ *Report of the Commission on the Social Sciences: Conclusions and Recommendations* (New York, 1934), 11, 51–52.

⁴⁴ Charles T. Fitts and Fletcher H. Swift, *The Construction of Orientation Courses for College Freshmen* (Berkeley, 1928), 169. Most sources recognize the Amherst College course "Social and Economic Institutions" in 1914 as the first general education offering; Fitts and Swift have found earlier beginnings in a "College Life" class at Pomona College in 1911.

⁴⁵ Ernest H. Wilkins, "Initiatory Courses for Freshmen: Report of Committee G," *Bulletin of the American Association of University Professors*, 8 (1922): 350–80.

There, in 1931, began a truly radical departure in general education: an undergraduate college for freshmen and sophomores with an integrated curriculum on comprehensive areas of knowledge—no disciplines, no departments, and, deep in its educational philosophy, no assent to the claims of history as the prince of unity. This rejection of history as an integrating discipline obstructed the coming of the Western Civ course until after the Second World War. Success came easier elsewhere, but the eventual triumph of the course at Chicago, where everything was against it, is more revealing of the real force of Western Civ, the influence of its support system in secondary schools, and the fundamental place of the history survey in general education.

From the start, Chicago had planned to do something different with freshmen and sophomores. Founding President William Rainey Harper, judging them to be unready for “strictly university work,” wanted to stick them someplace out of the way. From this first principle of separate status came the Junior College, an “ill-begotten” institution that, into the early twentieth century, was used by university departments as a training camp for young faculty and graduate assistants. In 1923, for instance, about one hundred assistants held menial teaching positions in a variety of elementary courses, and staff turnover reached an annual rate of 40 percent.⁴⁶ With the general education movement rising across the country, early undergraduate education hit bottom at Chicago. To the rescue came Robert Maynard Hutchins.

Enemies noted that President Hutchins arrived at Chicago with the Great Depression of 1929. Supporters recorded that his coming reversed the tide of battle in a faculty debate on a “College Plan” to convert the Junior College to a compulsory general education curriculum. Great curricular revolutions, someone has claimed, require either a commanding personality above or a common faith below. At Columbia, the coming of general education required a moment of patriotic unity; at Chicago, it required a philosopher-king. Nowhere was the debate more divisive—or more “religious.” For Hutchins, the university, in its most lofty sense, ascended out of the common communion of undergraduate learning: “We can never get a university,” he insisted, “without general education.”⁴⁷

Observers recorded that the content of general education at the College of the University of Chicago was worked out not by Hutchins but by faculty and deans, sometimes in response to their president, sometimes in opposition to him.⁴⁸ All shared, however, what has been called “the Chicago idea.” Basic to it was the conviction that disciplines, divided by convention into independent departments, fell naturally into larger clusters, groups of subjects connected by similar methods of inquiry. From this was formed a core curriculum of courses in three comprehensive fields: the social sciences, the humanities, and the natural sciences. Ideally, instruction in these broad areas was intended not to transmit factual knowledge but to develop critical understanding of their distinctive organizing ideas and systems

⁴⁶ Reubin Frodin, “Very Simple, but Thoroughgoing,” in *The Idea and Practice of General Education*, 41.

⁴⁷ Hutchins, *The Higher Learning in America* (New Haven, 1936), 59. On the plan, see Chauncey S. Boucher, *The Chicago College Plan* (Chicago, 1935).

⁴⁸ Daniel Bell, *The Reforming of General Education* (New York, 1966), 26.

of logic. The result in the humanities was the so-called Aristotelian tendency to classify cultural expression in terms of conceptual categories. Artistic works were studied not within the flux of historical change, where everything was relative to time and place, but within genres, forms, or topics that were deemed to be timeless or recurring in human creative production. Wrote College Dean Clarence H. Faust, "The history of human culture, that is, the organization of an intelligible, chronological sequence of man's achievements in philosophy and the arts, has come to seem much less useful to general education than the careful study and discussion of excellent examples of these achievements."⁴⁹ Here, in theories of approach, was the conflict between history and the Chicago idea. And where the prospects for Western Civ were concerned, "approach" was everything. Without support for history as a mode of intellectual synthesis, Chicago was a foreign country. Where the New History did not go, neither, at this point, did Western Civ.

History department files at Chicago reveal the Hutchins years as a time of protest, distrust, and fear of subjugation. Resentment lingered, for example, after a wrangle in 1934 following unofficial circulation of a mimeographed essay by Professor of English Ronald S. Crane, founder of the Chicago school of literary criticism and—to historians—embodier of its antihistorical precepts. Crane argued that the New History, in attempting to annex all disciplines, in fact left historians no discipline of their own. In presuming to integrate subjects in which they had no expert competence, he remarked, historians became amateurs in a university of professionals. All disciplines, he concluded, study the past, and specialists know the technical history of their trade in a way that historians cannot: "Other things being equal, the only histories of thought that can be taken seriously will be those written by philosophers; the only good economic histories will be written by economists; the only good histories of art are those written by men trained in aesthetic analysis."⁵⁰ Where every discipline was its own historian, there was no longer a discipline of history. To Crane, nothing so revealed the poverty of the New History as its claim to "integration" and "synthesis." Writing sweeping summaries of the past, he contended, was the work for popularizers and required little more than common sense and practical experience.

Historians replied with meetings, manifestoes, and much "thrashing over of matters" among themselves.⁵¹ At a time when the New History emboldened historians elsewhere to expand the claims of their discipline, historians at Chicago had to defend their very existence in the university. Beginning on high, this debate soon spilled down into the College course in Humanities, where historian Ferdinand Schevill was working as course director to develop a distinctive Western Civilization approach. When introduced as a core course in the new general education curriculum in 1931, the class was based on an earlier offering in

⁴⁹ Faust, "The Problem of General Education," in *The Idea and Practice of General Education*, 23.

⁵⁰ Crane, "The Organization of History in a University" (1934), 8, University of Chicago, Special Collections Department [hereafter UC, SCD].

⁵¹ William T. Hutchinson, "The Department of History in Retrospect" (1956), 37, UC, SCD, Hutchinson Papers. For the department's response to Crane, see "The Objectives of a Department of History," UC, SCD, Louis Gottschalk Papers.

European history, now hollowed out in order to make room for material from other disciplines. The College catalog announced, "This course uses the materials of history as a foundation and framework for the presentation of the literature, philosophy, religion, and art of the civilizations which have contributed most conspicuously to the shaping of the contemporary outlook on life."

In the years before his retirement in 1935, Schevill, an old-school lecturer of considerable fascination, made the course into a popular attraction in the Chicago area; but thereafter staff members from other humanities disciplines began to close in on his successor. The course, they complained to historian Arthur P. Scott, was too historical. To them, history as a medium of integration in the humanities was a form of reductionism that failed to provide independent aesthetic standards by which students could evaluate cultural works. Everything, instead, was interpreted as an "expression" of historical time and place. Historians, they objected, were concerned with factors that conditioned literary and artistic creations; they, in contrast, were concerned with the value and truth of literature and art. Scott complained in response that history was being reduced to mere chronology and that those in humanities already monopolized the substance of the course.⁵² But, in fact, the eclipse of history was only beginning. In 1942 the course was converted to analytical methods more consistent with the Chicago idea, and history virtually vanished in the new "Four Year College," the radical Hutchins experiment combining high school juniors and seniors with college freshmen and sophomores in an integral general education program. In a school founded on faith in the unity of knowledge, no place remained for instruction in the unifying methods of history.

Instead, the crown course of the new curriculum was "Observation, Interpretation, and Integration" (OII), a class providing a final, philosophical synthesis of the whole four-year program. Recalled sociologist Daniel Bell, who worked for a time in the Hutchins College, "The OII course represented, to the extent that any single course can, the specific 'ideological' commitments of the Chicago curriculum."⁵³ Complaints increased, however, that students lacked historical knowledge and the "time sense." Finally, after much debate over this "history question," the faculty in 1946 authorized an experimental Western Civilization course to be placed opposite OII in the final year of instruction; Western Civ was to integrate the curriculum by the historical method, OII by the analytical principles proper to the Chicago idea. As a result, Western Civ not only entered by the back door but also stood isolated at the end of the curriculum, a "conclusion" to a program based on a different educational philosophy. In the encounter of Western Civ and OII, however, the New History and the Chicago Idea met head to head.

Decisive to the outcome was a return toward "normalcy" that followed the departure of Hutchins from Chicago in 1950. As his successors dismantled the more unconventional philosophical structures of the College, OII went adrift, foundered, and was eventually abandoned. Conversely, Western Civ, moored in

⁵² On this quarrel, see UC, SCD, Arthur P. Scott Papers, box 1, folder 3.

⁵³ Bell, *The Reforming of General Education*, 35. Also see William O'Meara, "Observation, Interpretation, and Integration," in *The Idea and Practice of General Education*, 232–45.

secondary school history classes and more familiar general education ideas, became securely fixed in the curriculum. The ironic conclusion, noted Bell, was that the course alien to the Chicago program became the most enduring.⁵⁴

The original Chicago version of Western Civ, however, was very different from the class at Columbia. Both were the products of committees in which representatives from various disciplines worked to compromise their differences. At Columbia, the members from the start accepted historical chronology as the organizing structure of the course; at Chicago, on the contrary, this was the most contentious issue of all. Here the analytical methods of the Chicago idea did battle with the genetic principles of the New History. Struggling during 1947–48 to form a majority among themselves, the committee members finally pieced together a course based on a topical or “problems” approach.⁵⁵ In form, selected topics ranging from the Greek polis to Bolshevik Russia were aligned in the syllabus like—in the phrase of one committee member—“a string of beads,” placed in chronological order but connected only loosely by the line of historical continuity. Exceptional—but necessary for the support of committee members in the humanities—was the insertion at various places along the “string” of special topics on cultural themes, such as the assignments on “The Representation of Space in Art,” which followed the topic on the Renaissance and Reformation. The overall design was to stress “analysis” over “genesis,” concepts over chronology, and the internal world of the topic over the process of historical change.

Furthering this purpose was the emphasis—traditional at Chicago—on source readings and discussion methods in the course (one common lecture and three discussion sections per week), and committee opposition to the primacy of lectures and textbooks. Once in operation, however, the class appeared fractured by “discontinuities” and chronological gaps. In remedy, the teaching staff devised “bridge” lectures to connect topics, and staff chairman William H. McNeill authored a textbook to supply the historical narrative that was missing from the course. Thus the “story” of history was placed in the background, with students generally expected to reconstruct it on their own. In the original form of Contemporary Civ at Columbia, dominated by the evolutionism so central to the “progressive” outlook of James Harvey Robinson, textbooks carried the class, providing the central narrative on the central civilization in world history. At Chicago, however, where selected topics were the main focus, McNeill’s *History Handbook of Western Civilization* (1953) was improvised as a kind of manual for historical reference.⁵⁶

Many of these arrangements derived from the array of forces in the founding committee and the nature of the teaching staff in the early years of the course, when about half of the personnel were in fields outside history. Looking back, some historians remember Western Civ at this time as a course in bondage to other

⁵⁴ Bell, *The Reforming of General Education*, 36. Today the class continues as a required course (with options) for most sophomores.

⁵⁵ Deliberations can be followed in the records of committee member William T. Hutchinson of the Department of History; see UC, SCD, Hutchinson Papers, box 19, folder 5.

⁵⁶ McNeill has discussed the early course; see his “Integration: History of Western Civilization,” in *The Idea and Practice of General Education*, 225–32.

disciplines and antihistorical ideas.⁵⁷ Liberation came in the circumstances after Hutchins's exit in 1950, when those in other disciplines dropped away from the staff, the special cultural topics were removed from the syllabus, and the course moved under the protection of the Department of History. Significantly, however, the topical approach to subject matter was retained. A method of organization that historians once accepted as a matter of necessity they came to recognize as an effective strategy of instruction. The story of the Chicago course, for this reason, demonstrates an important development in the larger history of Western Civ classes after 1945: the quest for new principles of order.

James Harvey Robinson developed a general history extending across all human activity but narrowly limited by a perceived genetic principle of historical development. Incorporated into Western Civ Courses, this version of history, designed to help undergraduates find their bearings, soon lost its own. One reason was an inherent tendency in the New History to expand and sprawl; another was the deterioration of Robinson's principles of selection. The politics of the 1930s, for example, ravaged the assumptions of Robinson and his successors on the progressive course of events. Reviewers of Carleton J. H. Hayes's new textbook in 1936 noted, in this regard, that fascism and dictatorships in Europe had brought a strain of pessimism to his historical outlook.⁵⁸ This decline of faith in a central direction in history opened Western Civ courses to subject matter from all directions, to all of the "aberrant" events, lost causes, and silent peoples that Robinson had swept away with the clutter of the old general history. "We have probably put into these introductory courses by now all that they will hold," explained Smith College historian Sidney Packard in 1940; "the next step must be selection and omission, a process which calls both for judgment and courage."⁵⁹

Textbook writers confronted the same task. From the start, Western Civ traveled by textbook, and it was the success of particular works that helped standardize the course across the country. Indeed, the books spawned by Western Civ classes—a mass market in college textbooks if there ever was one—are a study in themselves. Condensed, the story is this: larger history begat larger textbooks. Some observers believed that the ultimate was reached in 1942 with Walter T. Walbank and Alastair M. Taylor's *Civilization: Past and Present*, two volumes of over one thousand double-columned pages, surveying the "history of man—his government, economic, social, religious, intellectual, and esthetic activities—from the earliest times to the present, in Europe, Asia, and in the Americas." After this, remarked a reviewer, the mission was to cut and slash.⁶⁰ Thus, by the start of the Second World War, both teaching and textbooks in Western Civ were in need of new methods to relieve the congestion of material caused by a deterioration of the old approach.

⁵⁷ Interview with former course chairman Karl J. Weintraub, September 26, 1979.

⁵⁸ See Harry Elmer Barnes, Review of Hayes's *A Political and Cultural History of Modern Europe*, 2 (New York, 1936), in the *American Sociological Review*, (1936): 514–16.

⁵⁹ For the results of Packard's survey of sixty college courses, see his "The Introductory College Course in History," *Social Education*, 9 (1940): 538–53.

⁶⁰ Thomas C. Mendenhall, "The Introductory College Course in Civilization," *AHR*, 49 (1943–44): 681–84.

This process, carried through after the war, was influenced by a profound change in methods of intellectual inquiry that overspread all areas of knowledge. Scholars have described it as a “conceptual revolution,” a shift from modes of investigation developed to verify facts and processes fixed in reality to the construction of conceptual models used to interpret a world in which all knowledge was relative to the knower. In historical studies, this involved the decline in evolutionary concepts of development and, as a result, the depreciation of historical narrative as the primary mode of explanation. Robinson himself once pondered whether an expanded history, having gained the whole world, would lose its soul. For him, the calling of the historian was to describe “the process of life and change.”⁶¹ Now some of the best historical minds came to see this style of historiography as an expression of nineteenth-century evolutionism. In process was a fundamental reorientation in which history opened to the social sciences and methods of conceptual analysis, and descriptions of genesis and growth made way for the study of social phenomena, ideological systems, and the “structure” of historical periods.

Where these ideas trickled down into Western Civ courses, many instructors moved independently toward the forms of instruction that characterized the Chicago class. At Columbia, a postwar observer noted a “striking change” in the Contemporary Civilization course (CCA). No longer was the stress on historical continuity, but on the social and intellectual configuration of particular epochs, with little attention to the details of transition from one time period to another.⁶² In sum, Robinson’s history had been traded for a “string of beads.” Standard textbooks were replaced by a volume of background readings, and, after 1945, all instruction converted to discussion of source materials.⁶³ Half a century earlier, in “the great debate over the source method” at the AHA in 1897, historians upheld the supremacy of textbooks over primary materials and knowledge of the historical process over the habits of critical analysis engendered by the source method.⁶⁴ Now, at some schools, scholars were reversing the order of things.

Western Civ was passing beyond the conventions of a no longer New History. Being left behind as well, however, was the original organizing idea of the course. As new documents collections, “problems” pamphlets, and case studies involved students in “doing” history rather than simply learning it, and as the source method and topical approach fragmented the sense of historical continuity, the unifying idea of genetic development was diminished. Wrote Daniel Bell of developments in Contemporary Civ at Columbia, “What was gained by the addition of original material was, in part, offset by the lack of a consistent interpretative framework.”⁶⁵ More importantly, such Western Civ courses, in departing from instruction in a

⁶¹ Robinson, *The New History* (New York, 1912), 65–66.

⁶² Charles H. Russell, “The Required Programs of General Education in the Social Sciences at Columbia College, the College of the University of Chicago, and Harvard College” (Ph.D. Dissertation, Columbia University, 1961), 60.

⁶³ On changes in CCA from 1937 to 1946, compare Carman, “An Example of an Integrated Program in the Social Sciences,” 250–51, and *A College Program in Action*, 95–103.

⁶⁴ Robert E. Keohane, “The Great Debate over the Source Method,” *Social Education*, 13 (1949): 212–18.

⁶⁵ Bell, *The Reforming of General Education*, 212.

common body of knowledge, became at the same time less defensible as general education.

Events at Chicago reaffirmed the central position of history in general education. Already, however, Western Civ courses were living on credit. Therefore, when general education entered a period of crisis in the 1960s, it was inevitable that Western Civ would follow. Having traced the rise of the course in connection with this movement for a common learning, it remains now to trace its decline.

THE SECOND WORLD WAR GAVE A SECOND SURGE to the general education movement.⁶⁶ Members of President Truman's prestigious Commission on Higher Education, confronting a human wave of college-bound war veterans, recommended in 1947 federal funding for more of everything on campus; but general education, they concluded, was the really "crucial task" for American colleges. "In the nation," exclaimed the dean of the College of the University of Chicago in 1950, "general education is at last in vogue. Its principles bid fair to become the operative educational theory of the remainder of our century."⁶⁷

Not so. War veterans brought a new seriousness to college programs. In 1957, *Sputnik* revealed the new seriousness of the Soviet challenge in education. In the name of higher standards, specialization returned in force, departments raised up honors programs and freshman seminars, and core programs began to break apart. The Department of History's "take-over" of Western Civ at Chicago was but one episode in the waning of the general education ideal. Another was the dissolution of CCB, after thirty years' travail, as a sophomore requirement at Columbia in 1959, when social science departments objected that their vocabulary had become too technical for general education courses.⁶⁸ These developments, however, were only part of much broader changes in general education requirements at both schools, in which restraints loosened, electives returned, and areas of specialization were introduced. In brief, the elective system was breaking through the lines of general education, threatening the dogmas that lay at the center of its educational philosophy: the unity of knowledge and the ideal of a common learning. This challenge to general education meant a challenge to Western Civ as well. For connected to the absolutes of general education were the absolutes of the original Western Civ idea: the belief in the oneness of history, in the potential of the historical method to integrate human experience, and in Western history as the "high history" of mankind.

The coming crisis was forecast in developments at Harvard after 1945. There the forces of general education, after having conquered the very fortress of the elective system, were thrown into confusion at the moment of their greatest success. Moved

⁶⁶ See, for example, Earl J. McGrath, "The General Education Movement," *Journal of General Education*, 1 (1946): 3.

⁶⁷ *Higher Education for American Democracy*, 49; and F. Champion Ward, Foreword to *The Idea and Practice of General Education*, v.

⁶⁸ "Report of the President's Committee on the Contemporary Civilization Courses in Columbia College" (October 1960), Columbia University, Columbian Collection.

by the experience of the Second World War, the Harvard faculty in 1945 approved in principle a committee report that thereafter became scripture in the general education movement. *General Education in a Free Society* (dubbed the Redbook by virtue of its Harvard crimson cover) anointed general education with the fair name of Harvard, giving it a legitimacy that convinced college faculties across the country of the value of a common learning. But the Harvard faculty had second thoughts. The Redbook plan for compulsory core courses in the social sciences, humanities, and natural sciences gave way in debate to a “temporary” experiment permitting two to four optional courses in each of these areas. With time, what was temporary became permanent, and two to four options became more, and more. The elective system, thrown out the door, came back in through the window. Thus, from the start, the object of the whole exercise—a common educational experience—was lost.

Lost as well was a planned Harvard version of the Western Civ class, projected in the Redbook as the core course in the social sciences.⁶⁹ In rejecting the course, Harvard faculty members, in principle, rejected the historical pre-eminence of Western man. “To center a course exclusively on the Western tradition,” objected one opponent, “would merely help to perpetuate the old myth of civilization as a monopoly of the regions bordering on the Atlantic.”⁷⁰ Nor, objected others, was the historical approach sufficient to encompass the social sciences. When, therefore, two optional courses were approved, Western Civ at Harvard died without seeing the light of day. The options lived and multiplied. From two freshman-sophomore general education courses in the social sciences in 1949, the number increased to four courses by 1950, seven by 1960, and fifteen by 1970. An educational philosophy of unity and compulsion thus led to a curriculum of diversity and choice. The general education catalog, cracked a Harvard dean, was beginning to resemble the Manhattan telephone directory.

This breakup of an educational creed coincided with the breakup of the world that inspired it. Much of the deep structure of general education lay in a psychology developed during a half-century of U.S. involvement in the “crusade for democracy” in Europe. In logic that mixed patriotism and pedagogy, educators equated core courses with common values, the need for unity in the republic with the need for unity in the curriculum, and the Western military alliance with Western civilization. With the passing of the Cold War, however, there passed also the illusions that sustained general education as a civic religion. America’s new hegemony in the West eroded earlier notions of a common partnership with Europe, and the rise of the Third World confronted the United States with an international environment of polycentrism and cultural diversity. Europe was no longer the world. Emerging were other peoples, other histories, a globe of historic diversity beyond the imagination of earlier Westerners, a cosmos where pluralism replaced the “oneness” of history and where human experience could not be

⁶⁹ The course “Western Thought and Institutions” is described in *General Education for a Free Society*, 213–17.

⁷⁰ For a review of debate on the course, see David Owen, “Harvard General Education in Social Science,” *Journal of General Education*, 5 (1950): 17–20.

ordered into a unilineal pattern of development. As educators came to recognize the world in this way, they recognized, at the same time, the poverty of the Western Civ course.

THE DECLINE OF "HIGH HISTORY": AMHERST, 1954–66. As noted earlier, the Harvard approach to large classes was to station a dominant figure at the center of things, to identify the course with his personality, and to unite the class from the lecture platform. In the Western Civ course at Amherst College, that dominant figure was Laurence Bradford Packard. During his graduate student years at Harvard from 1909 to 1913, Packard was a teaching assistant to Charles Homer Haskins in History 1 and thereafter was one of those who spread the influence of this famous course to other schools. Packard would gladly teach. His reputation as a dynamic lecturer was already made—to the neglect of research and publication—when he came to Amherst from the University of Rochester in 1927, and immediately he established his new European history survey as the most popular elective course in the college. Within a short time, it was attracting over 80 percent of all freshmen.⁷¹ When the general education wave hit Amherst after World War II, therefore, Packard's class was converted in 1947 into a compulsory core course in the social sciences: "The Development of Western Civilization: A Survey of European History and an Introduction to the Social Sciences."

Packard loved the course, taught it every year, and never wanted to leave it. He was, in this sense, one of the exceptions to the rule: professors voted to compel general education courses but declined to teach them. But Packard was little moved by the spirit of general education. Ideally, all courses in the Amherst program were to conform to the "big ideas" of the general education curriculum of 1947: (1) the integration of disciplines, and (2) the introduction of discussion methods in small-group situations.⁷² Packard's class, however, true to its roots in the old History 1 at Harvard, remained largely a history survey, with only a sprinkling of subject matter from the social sciences.⁷³ Nor was Packard a man given to discussion methods—either in the classroom or at department meetings. Therefore in Western Civ at Amherst (three lectures and two discussion-section meetings per week), interdisciplinary analysis was subordinated to historical narrative, discussion methods to the lecture system, and the section staff to the lecturer. Packard held the ship on course, the others worked below deck.

In the manner of the great men at Harvard, Packard gave his class a consistency and continuity of direction that sustained it through changes in historical thought and curricular philosophy. Stated differently, the course grew old with him. For others involved, therefore, Packard's retirement in 1954 was a liberation. The course changed radically in the following years as a succession of instructors tried

⁷¹ For information on Packard, I am indebted to his friend and colleague Professor Alfred F. Havighurst of Amherst. Also see Havighurst, "Remarks . . . on the Occasion of the Announcement of the Laurence B. Packard Memorial Collection in the Robert Frost Library, April 30, 1977," Amherst College Archives.

⁷² On the curriculum, see "Report of the Faculty Committee on Long Range Policy," in *Education at Amherst: The New Program* (New York, 1955), 3–176.

⁷³ "Report of the Review Committee on the New Program," in *Education at Amherst*, 218–20.

new approaches. In the end, however, the ship went down with its captain. During 1962–63, an influx of young specialists in non-Western areas of history came to represent new interests within the Department of History. First, they wanted to extend coverage in Western Civ to other civilizations; finally, they wanted to scrap the class altogether. In 1966, as a retreat from general education spread across the nation, Western Civ at Amherst was replaced by a historiography course designed by these new men to be “innovative,” elective, more practical—and less Eurocentric. “Traditional courses left us dissatisfied,” one of them explained. “There are, of course, telling arguments for acquaintance with some portion of the sweep of human development, . . . but there seem to be no overwhelmingly convincing reasons for choosing one period or area over others as the introduction to history.”⁷⁴

Thus lapsed at Amherst the notion of the European past as “high history,” a development central to the human experience. Elsewhere, observers noted similar developments: the “intellectual capital” of the Western Civ course was being used up.⁷⁵ So was the moral capital. The American way of war in Vietnam caused many students to ponder the pre-eminence of Western history. At the 1976 AHA session on the Western Civ class, Amherst historian Frederic L. Cheyette bid good riddance to the grand old course. Despite its claim to be universal, he asserted, Western Civ in truth was limited and provincial, a history of those who were men, white, Christian, and European. Others had entered history, not merely the forgotten people of the Third World, but the newly discovered peoples of Europe as well: women, children, Jews, and peasants. “There is not *a* history,” Cheyette remarked; instead, there are “many possible histories.”⁷⁶

Nor, further, is there *a* Western tradition. Cheyette complained that teachers of Western Civ had merchandised a fixed and idealized conception of the European past. Their method was to lift ideas and events out of history, standardize them, and declare them to be “representative” of trends and values inherent in the Western experience. Illustrative, perhaps, is an Amherst freshman’s description of proceedings in Laurence Packard’s class in 1948:

In our history course we would not be concerned with the fall of the Bastille as a particular event at one place in time, but as a manifestation of a revolutionary spirit that later overwhelmed all of France. Monarchist government was on the way out at that time, and we speak of the Bastille as an event illustrative of a trend of resistance toward the monarchy. Our method of dealing with historical events is one in which we relate them in terms of trends of political, social, and economic thought through the ages.⁷⁷

In any case, the purpose of the technique, according to Cheyette, was to propagate a perception of European history as the progress of liberty and reason. To his mind, this revealed the “socializing function” of Western Civ. It explained the place

⁷⁴ John B. Halsted, “Reading History: An Innovative Approach to the Teaching of Introductory History at Amherst College,” *AHA Newsletter*, 8 (1970): 16.

⁷⁵ Bell, *The Reforming of General Education*, 211.

⁷⁶ McNeill *et al.*, “Beyond Western Civilization,” 536.

⁷⁷ Danny D. Gustafson, “A Freshman Looks at the New Curriculum,” *Amherst Graduates Quarterly*, 38 (1948): 1.

of the course in the history of education as well as its origins in the sprawling, immigrant America of the early twentieth century. "Western Civilization," Cheyette concluded, "like other creations of the General Education movement, had as one of its many burdens the task of acculturating Jewish kids from the Lower East Side, the fair-haired children of mid-Western towns, and the heirs and heiresses of Los Angeles janitors—the new clientele of the expanding university system—into the world of upper middle class America."⁷⁸

Certainly the course was a celebration of Western culture. But not everyone will agree with Cheyette that it was a form of thought control. More serious, however, because more generally shared, was his premise that history was no longer "one." Here, where Western Civ and general education came together, the consequences were plain. When there is no longer *a* history, when history ceases to have a central narrative, there is no longer a logical necessity for students to know either the same facts or the same history. The conclusion is this: just as, not long ago, scholars of the classics could not make a special case for the high culture of Greece and Rome in the college curriculum, so historians can no longer make a special case for the "high history" of Western civilization.

THE DISESTABLISHMENT OF GENERAL EDUCATION: STANFORD, 1967–69. In the same year that Western Civ died at Amherst, Daniel Bell presented to the Columbia faculty his brilliant report on the need to renovate the general education program. It succumbed to similar causes. "Through some persuasion of the *Zeitgeist*," commented a Columbia scholar, "the majority of the faculty were no longer concerned with general education in the large and honorific meaning of the phrase."⁷⁹ Across the nation, as students rose against requirements, they encountered faculties already prepared to retreat. Spirit had gone out of the general education ideal, and deans and secretaries were left to enforce the dead letter of core course requirements. At Stanford University, professors called it "general education by the registrar's office."

This crisis of general education was particularly devastating at Stanford as a result of a university self-study carried out at the most extreme stage of campus unrest during 1967–68. At a time when other schools tried to protect their programs from the storm, Stanford opened all of the windows. The national press described the self-study report, *The Study of Education at Stanford*, as "the most comprehensive effort to find new answers" to curricular problems. Today, however, it reads like a historical document, a manifesto of the 1960s, filled with the rhetoric of "relevance," liberation, and self-expression. "Let the objective of curricular planning be to encourage the faculty member to teach what he likes to teach," the report concluded, "and the student to learn what seems vital to him."⁸⁰

⁷⁸ McNeill *et al.*, "Beyond Western Civilization," 534.

⁷⁹ Lionel Trilling, "The Uncertain Future of the Humanistic Educational Ideal," *American Scholar*, 44 (1975): 57–58. Bell's report, *The Reforming of General Education*, was not acted on by the Columbia faculty.

⁸⁰ *New York Times*, August 10, 1969, p. E-7; and *The Study of Education at Stanford* [hereafter, *SES*], 10 vols. (Stanford, 1968), 1: 14.

Against this return to “curricular do as you please,” the Western Civ course could not stand.

Thus the fall of Western Civ at Stanford was inseparable from the fall of general education. The course there had been the pride of the general education curriculum (called the General Studies Program), and, as a requirement for almost all freshmen, was virtually a rite of initiation to the university. “Civ is the General Studies course *par excellence*,” observed a faculty member in 1967. “As we conceive that course, so we conceive the whole general education of Stanford undergraduates. If Western Civ were radically rethought . . . , the entire structure of General Studies would have to be rethought with it.”⁸¹

Historically, the course at Stanford was formed in 1935 to replace a required freshman course in Citizenship, which, after patriotic beginnings in 1920, had run out of faculty support and student interest. Rather than building on existing offerings in European history, a committee of the Department of History designed Western Civ from models at other schools.⁸² Influential in these deliberations was a young department member who, as a graduate student at Columbia, had taught in the Contemporary Civilization course at Barnard College. Maxwell Hicks Savelle was the major force in bringing the ideas of the Columbia course to Stanford. In opposition to support for the lecture method, he won the main battle for the discussion approach (three discussion sections and one lecture per week). Thereafter, as director of the class through the first ten years of its existence, Savelle worked to promote the Columbia plan for staff power and independence.⁸³ Unlike the interdisciplinary staff of Contemporary Civilization at Columbia, however, the Western Civ staff at Stanford remained a satellite of the history department. Historians appointed the course director, delivered the weekly lectures, recruited graduate-student instructors—and made the course a smashing success.

In a committee report in 1956 that introduced the General Studies Program at Stanford, Western Civ was identified as the most popular course among undergraduates and its instruction methods recommended as a model for the new curriculum.⁸⁴ With this recognition came the financial resources to appoint full-time staff members from among some of the best graduate students in history available at universities across the country. They arrived just in time for the events of the 1960s. As the retreat from general education commenced, and as the Department of History relaxed surveillance over Western Civ, a staff of about twenty new men, holding temporary faculty appointments, came into control, year by year, of the syllabus, the lectures, and finally the course itself.

What followed is best understood in connection with the labor problem in general education. In a profession where advancement depended on expertise and publication in specialized fields, professors often preferred to leave the work of

⁸¹ Memorandum, Subcommittee on the History of Western Civilization, April 1967, Stanford University Archives, History Department Papers [hereafter, SA, HDP], carton 21, folder 361.

⁸² See the committee report, undated, in SA, HDP, carton 5, folder 126.

⁸³ On Savelle's dedication to the course, see George H. Knoles, “The History of the History of Western Civilization,” *Stanford Observer*, April 1980, pp. 4–5.

⁸⁴ Robert Hooper and Hubert Marshall, *The Undergraduate in the University: A Report to the Faculty by the Executive Committee of the Stanford Study of Undergraduate Education* (Stanford, 1957), 96–97.

general education to an academic proletariat of temporary personnel and graduate teaching assistants. At many schools, the "TA" became a classic figure of the Western Civ class, at once sustaining the course and limiting it, bringing the benefit of cheap labor and the problems of inexperience and staff instability. In part, methods of instruction at Harvard were developed to give substance and support to a class maintained by unpracticed assistants. "The section men have little effect on the enjoyment of the course," a confidential course guide counseled freshmen concerning History 1 in 1934, and opinion polls generally recorded that students judged discussion sections to be less important than lectures.⁸⁵ In styling its Western Civ course to the Columbia pattern, however, Stanford adopted an ideal of instruction based not on the charisma of the lecturer but on the chemistry of the discussion section.

The Columbia system, in fact, was developed in reaction to the "display of personality" involved in the mass-lecture method and the use of green, ever-changing graduate assistants hired on the cheap. If, at Harvard, the class "belonged" to the lecturer, at Columbia it belonged to the staff, a team of regular faculty members sharing conviction in the discussion approach. Declared Dean Harry Carman in 1946,

For many years we have given in Columbia College no required courses of the pontifical type, in part because the students know the defects of the type, but principally because the man-to-man effectiveness of a proved instructor, young or old, with a small group . . . has had much to do with the active undergraduate interest in the introductory work, and with the easy and steady improvement of the courses themselves.⁸⁶

To promote consensus, democratic debate and "workers' control" were made the mode of staff work at Columbia. The Contemporary Civ class, in consequence, was a course in motion, geared for change and self-correction, with committees of students and staff involved in constant review and revision. "Contemporary Civilization was literally born revising itself," wrote a faculty member in 1959. "There has always been and there will always be a Contemporary Civilization Revision Committee."⁸⁷

But, just as the course was strengthened in the early period by the shared beliefs of its staff, so in the 1960s was it weakened by the loss of faith in general education. A report of 1960 describes the condition of the staff as "alarming," with morale low, turnover high, ranking professors few in number, and various junior instructors merely going through the motions. Various observers have remarked that such staffing problems were as responsible as intellectual problems for the crisis in general education courses; and Daniel Bell noted that intellectual problems were sometimes staffing problems in disguise.⁸⁸ In retrospect, however, it appears that

⁸⁵ *Harvard Crimson Confidential Guide to Freshman Courses*, 1934, p. 22, HUA.

⁸⁶ *A College Program in Action*, 23–24. On criticism at Columbia of the "wholly deplorable" results of the strict lecture method, see Lionel Trilling, "The Van Amringe and Keppel Eras," in Miner, *A History of Columbia University*, 40–41.

⁸⁷ Buchler, "Reconstruction in the Liberal Arts," 57.

⁸⁸ "Report of the President's Committee on the Contemporary Civilization Courses," 2–3; and Bell, *The Reforming of General Education*, 66.

the Contemporary Civ staff at Columbia did not begin to spin apart until after centrifugal forces built up within the course itself. When textbooks were abandoned and new methods of history developed after 1945, source readings proliferated and the syllabus began to clog. After criticism in 1957 that the gathering disorder was giving students "misleading conceptions" of scholarship, a trend developed within the staff toward individual experiment and personal initiative. Workers' control converted to private enterprise. The ideal of a common syllabus—honored since 1919—gave way in 1964 to the expedient of individual selection in reading assignments. In 1968, the famous, common source book *Introduction to Contemporary Civilization in the West*, used since 1946 and widely adopted at other schools, was replaced by a choice of paperbacks. So, after a half-century, the commonality of learning that had made the Contemporary Civilization course at Columbia the inspiration of the general education movement was lost. If the Harvard system risked limiting the syllabus to the intellectual perspective of one man, the Columbia system risked making it all things to all staff members. One system ran the danger of authoritarianism, the other of anarchy.

By 1967, when the self-study of the General Studies Program at Stanford began, anarchy was already well advanced within the Western Civ staff. As a staff member during 1963–66, the present writer witnessed the process of disintegration. The discussion section was the world of the staff, and, left to ourselves, we reconstructed the course in its terms, disconnecting everything that was not directly involved in our work there. Textbooks were demoted to reference materials. Lectures, largely ignored in section discussions (and poorly attended by students as a result), were reduced in number and finally, when instructors began to stay away as well, eliminated altogether. Just as, at Amherst, the reign of the lecturer resulted in the subordination of discussion sections, the reign of the staff at Stanford resulted in the subjugation of the lectures.

"The instructors sabotaged the lectures," a member of the history department complained sometime later.⁸⁹ But no one spoke at the time. In effect, the major undergraduate course in the university was turned over without challenge to the most junior faculty members. For us, the road was open; it led to experimentation, eclecticism, and the same kind of "personal preference" syllabus that ended in disorder at Columbia. We thought, in teaching from personal perspectives, that we were deepening the course. Instead, we were digging its grave. Irresistibly, the impression spread among students that the class was in confusion. Under the cover of one course, a freshman complained to the campus newspaper, different instructors taught different things.⁹⁰

Like the students of these protest years, the members of the self-study committee had a way of asking hard questions. What goals, they wanted to know, united a class taught in such separate ways? Why, if Western Civ was no longer a common educational experience, did it remain a common requirement? And what, precisely, was that body of historical knowledge that all educated men should know? As

⁸⁹ Department memorandum, 1968, SA, HDP, carton 21, folder 361.

⁹⁰ *Stanford Daily*, May 17, 1966.

individuals, historians replied differently to these terrible questions; as a department, they did not even try. Other faculty members watched in puzzlement as, throughout the proceedings, the Department of History remained silent in the face of assaults upon the course. "They think it curious," remarked the department chairman to his colleagues, "that the Department has neglected to take a more vigorous stand."⁹¹ But the compulsory class, it seemed, was no longer worth fighting for. "First of all," wrote Western Civ course director Paul S. Seaver, "many freshmen are unhappy with the requirement and, lacking motivation, find the course a source of frustration. . . . Secondly, many of us as faculty are no longer convinced that there is a standard or specifiable body of knowledge or information necessary for a liberal education."⁹²

A poll in 1968 revealed that 59 percent of the students in Western Civ believed that the course should be a university requirement.⁹³ Too many historians, however, had lost faith. Accordingly, the Department of History in the following year did not oppose a recommendation to discontinue Western Civ as a required course. Thus ended, after thirty-four years, the privileged place of history in the education of all undergraduates at Stanford. Further, the disbanding of the course coincided with the disestablishment of the General Studies Program as a whole. Faculty opinion had turned against massive courses on massive subjects, courses that pretended to all-inclusiveness, to interdisciplinarity, that attempted "to squeeze the universe into a ball and roll it toward some overwhelming question."⁹⁴ Instead, small was beautiful. The new ideal of introductory instruction at Stanford was the freshman seminar, classes that were small, elective, limited in discipline, varied in subject matter. In a phrase, they were the antithesis of general education. *The Study of Education at Stanford* concluded,

General education, as epitomized by the Chicago curriculum of the Hutchins era and the Columbia two-year sequences in Humanities and Contemporary Civilization, is dead or dying. The Harvard general education ideal, as defined in the famous Redbook of 1946, did not ultimately flourish in its own birthplace. . . . The general education ideal is totally impracticable as a dominant curricular pattern in the modern university.⁹⁵

SOMEONE HAS SAID THAT ALL CURRICULA ARE ESSENTIALLY RELIGIOUS. At Stanford—and many other locations as well—faith in the religion of a common learning broke in the 1960s. "General education in America is now a disaster area," lamented the Carnegie Commission on Higher Education in 1977.⁹⁶ Today, as noted at the outset, educators are attempting to unify the curriculum again. Educators too,

⁹¹ David M. Potter, department memorandum, January 29, 1968, SA, HDP.

⁹² Seaver, letter of September 10, 1968, reproduced in *SES*, 2: 75–76.

⁹³ *Stanford Daily*, January 27, 1969.

⁹⁴ See the Report of the Committee on the General Studies Program of the Faculty of Humanities and Sciences, 1967, printed in *SES*, 2: 85–89.

⁹⁵ *SES*, 2: 9, 24.

⁹⁶ Carnegie Commission, *Missions of the College Curriculum: A Contemporary Review with Suggestions* (San Francisco, 1977), 11.

however, can return from a revolution having learned nothing and forgotten nothing. The curriculum cannot be ordered back together in the old way. Compulsion was inherent in the old idea of a common learning: what all educated men *should* know, all students *should* be required to study. Critics have described this as a philosophy of “education without students,” a utopia of the academic mind in which the young are molded to a faculty image of “the educated man.” When, in the last century, the elective system first gave students a voice, they swept away the old classical curriculum; when they spoke again in the 1960s, they nearly demolished the general education curriculum as well. Now, in reaction, wiser faculties are reviving general education in forms less rigid and doctrinaire. Observers at Harvard, for example, have noted that the new core curriculum there is neither a core curriculum nor even very new, but rather a more coherent pattern of electives.⁹⁷ Inevitably, this transition to more flexible forms of general education involves a transition as well to more varied forms of freshman history.

Thus arises again what Charles Homer Haskins described in 1905 as “the most difficult question which now confronts the college teacher of history”: the nature of the first-year course. Here all thinking must begin afresh. “There is, as I see it,” Harvard historian Giles Constable commented wisely at the AHA session on Western Civ in 1976,

no ideal survey course, of which the model has somehow been lost in the confusion of the past decade and which is waiting to be resurrected for the next generation. Western civilization succeeded admirably for the period for which it was designed, but each generation must construct its own introduction to history in terms both of the scholarly nature of the subject and the needs and interests of students at that time.⁹⁸

Nostalgia is likely to remain. The fathers of Western Civ will be venerated for resisting the fragmentation of knowledge, for calling specialists to the task of teaching general history, for involving freshmen in discussion of source materials, and for implanting history in a paramount place in undergraduate education. But it is easier to bury the course than to praise it. Most historians have long concluded that the world has outgrown the old Western Civ ideas. Others will be so instructed by the story told here. Truly those who do not learn from the history of this wilted course are doomed to repeat it.

⁹⁷ Harold Taylor, *Students without Teachers: The Crisis in the University* (New York, 1969), 156; and Adele Simmons, “Harvard Flunks a Test,” *Harper’s Magazine*, 258 (March 1979): 20–22.

⁹⁸ McNeill *et al.*, “Beyond Western Civilization,” 532. Historians discussed the introductory course again at the 1980 annual meeting.

Comments:

AT A TIME WHEN MANY ACADEMIC INSTITUTIONS are engaged in the periodic rite of curricular revision, historians willing to apply their own precept that an understanding of the past is pertinent to policymaking will find invaluable the context provided by Gilbert Allardyce's retrospective on the Western Civ course and the general education movement that encased it. Allardyce has thoroughly researched and thoughtfully narrated the life course of five program paradigms that were widely influential in disseminating the Western Civ model as the basic history and humanities course in American colleges and universities. And his analysis of the political and pedagogical determinants of Western Civ draws particular attention to often unspoken premises and implications of curricular decisions, which at once draw upon and in turn shape developments beyond academic walls. Allardyce concludes that Western Civ is dead and buried, but he stops short of offering an agenda for developing its successor. Indeed, Allardyce's description will yield a prescription for new paradigms appropriate to changed political and pedagogical contexts only if it is supplemented in two ways: with a more accurate diagnosis of the patient's current health and with the addition of cultural determinants to political and pedagogical ones.

PROFESSOR ALLARDYCE'S DIAGNOSIS IS INCOMPLETE because the article is already significantly outdated. Whereas he ends his sustained exposition with the 1976 AHA panel entitled "Beyond Western Civ," there have been several important developments in the past five years, some of which have modified the trends Allardyce traces. In September 1980, for example, the AHA initiated a comprehensive and systematic study of the introductory course with a conference for practitioners held at Annapolis. Several panels at AHA annual meetings since 1976 have discussed ways of teaching Western Civ, and at the 1980 meeting a panel probed the conceptual issues raised by the impact of women's history on the general studies curriculum.¹ Such occasions have shown, among other things, that Allardyce's report of Western Civ's demise is exaggerated. Even though, as he

¹ The 1980 session was entitled "The Place of Women's History in the Rebuilding of the General Education Curriculum," and the participants included William H. Chafe, Sara M. Evans, Mary Gordon, Carolyn C. Lougee, Amy Swerdlow, and Martha Tolpin. At the same meeting there was a panel entitled "The Teaching of Non-Western History at the Secondary and College Level," and its participants included Michael P. Adas, Philip D. Curtin, Nancy Henningsen, David Johnston, and Winthrop R. Wright.

demonstrates, the five paradigmatic programs did disintegrate, these five institutions no more sum up American academia than Europe sums up the world; the Western Civ course in many places lives on. More than this, even at one of the five institutions Allardyce profiles—Stanford University—Western Civ (with only minor administrative and staffing changes) was reinstituted in 1980 as a university undergraduate requirement—testimony to the strength, for better or for worse, of the Western Civ conception as an ideal in the minds of those who would re-establish a common learning experience at the center of undergraduate studies.

Is this Thermidor? What kind of corpse do we actually have on our hands when the winds of revision prove still capable of resuscitating it? Contrary to Professor Allardyce's assessment, it is proving at least as hard to bury the course as to praise it. Clearly, more than nostalgia remains. Indeed, from my perspective the challenge appears to be less the luxury of designing new courses from scratch than the necessity of keeping the corpse from being resurrected intact. In this endeavor Allardyce's analysis of the rationale and roots of the old model can be of crucial assistance, especially for an understanding of the model's time specificity, the way it grew from a particular set of national and international needs. But Allardyce misses the model's cultural roots, which constitute an important key to the old conception, to the reason it (temporarily) collapsed, to the reason it is now being revived, and by extension to the indispensable precondition of vitality in its eventual replacement.

It was no accident that the Western Civ course was conceived at a particular point in American social development, for its focus on "grand ideas and great books," on "men of influence in human affairs" (pages 695, 696) promoted social and cultural assimilation, "the melting pot." It was not only the commonality between Americans and Europeans inspired by the First World War that stood at the core of the Western Civ ideal; it was also the commonality between Poles and Irish, between English and Italians, among all of the European immigrant groups pouring into America, some of which were sending their children to college. The Western Civ course portrayed European civilization as a unit transcending ethnic borders, as a distillation of a civility that supposedly marked the best in each country and to which the educated of whatever origin could aspire to assimilate through humanistic education. In fostering a standard of "common European life" underlying national differences,² the Western Civ ideal operated as the Latin language and the entire *studia humanitatis* long had: Western Civ and the general studies movement underlying it had, then, a philosophy of unity and commonality with respect to culture, just as Professor Allardyce shows it had with respect to knowledge and disciplines. In cultural terms, the Western Civ ideal was homogenizing and normative: it socialized the young from whatever particularist background traditions to a uniform standard of thinking and behaving that ought to characterize America's expanding educated class.

Like consensus history, to which it was tied, this normative Western Civ worked until the efflorescence of the 1960s. What changed in the past two decades included

² J. Franklin Jameson, "The Development of Modern European Historiography," *Atlantic Monthly*, 66 (1890): 322.

the political and pedagogical, as Professor Allardyce suggests—the shift in global power relations that changed the place of the West in the world and the advent of the pedagogy of *laissez faire*. But an unmentioned solvent of at least equal strength was *cultural* development *within* America. The resurgence of self-consciousness and ethnic identification among old groups supposedly melted down and newer arrivals as well as the gender group of women challenged the cultural model of conformity to a male European norm with a vindication of cultural difference and establishment of minority cultural studies as academic disciplines. Similarly, developments within disciplines complemented the flowering of particularist cultures within American society, directing attention to numerous levels of culture: not merely the *Kultur* of the “great ideas and great books” but also popular culture, mass culture, countercultures; and not merely culture in a public sense but culture as patterns in private relations. As a consequence, the generation now designing new, model introductory history courses is separated from the architects of Western Civ by a different understanding of civilization, one that embraces the Cathars of Montaillou as well as the Scholastics of Paris, the mothers of families as well as Founding Fathers.

What has preserved Western Civ courses through the years and is now bringing them back has been not merely a pedagogical preference for structure over chaos in courses of study, in what Professor Allardyce shows to be a remarkable reprise of post-1900 responses to curricular fragmentation, but also a rejection of confusing cultural fragmentation, a rejection of competing claims of diverse cultures with different standards of value, a desire to recapture a firm common canon that can endure in the face of proliferating particularist traditions. And yet here is precisely the crucial point, the point where the old Western Civ course is most inappropriate for the closing decades of the twentieth century. For the common illusion of Western Civ courses has been their premise of uniformity, commonality, and the public. As Allardyce demonstrates, Western Civ and the general education movement alike aimed at intellectual unity: “shared values that made possible an enlightened public consensus. General education, therefore, was a philosophy of unity: unity of knowledge, unity of the curriculum, unity of education and life” (page 698). But the unity was also to be cultural, and this ideal of cultural unity has been shared even by those who opposed or modified this tradition, from President Eliot of Harvard on down to Dean Rosovsky with his lament over the loss of “an older community of beliefs.”³ Even William H. McNeill is part of the problem rather than the solution in saying that what we need to teach is “something all educated persons should know; something every active *citizen* ought to be familiar with in order to conduct *his* life well and perform *his public* duties effectively.”⁴ Central to the move from the old conception to a new vision will be the need to

³ Henry Rosovsky, “A Letter to the Faculty on Undergraduate Education, October 1974,” Harvard University Archives, Cambridge, Mass.

⁴ McNeill *et al.*, “Beyond Western Civilization: Rebuilding the Survey,” *History Teacher*, 10 (1977): 510 (italics added).

understand community life as more than public, as a continuum of private and public, and to define the audience as adults rather than merely citizens, as men and women rather than merely men.

AND SO THE CHALLENGE, it seems to me, which is at once clarified by Gilbert Allardyce's exposure of the political and pedagogical dimensions of the Western Civ concept and obscured by his failure to probe its cultural premises, is to formulate a vision of coherent diversity, of cultural pluralism. Whether institutions establish a new, single common course or not, whether as an undergraduate requirement or not, whether history departments base their budgets, billets, and majors' recruitment on it or not: these are relatively superficial issues. The important challenge is to develop introductory history courses that incorporate the richness of particular cultures, whether on a global scale or within a nation or region, to conceive of humanities courses that can (as the social sciences can) accept and even celebrate diversity, the eccentric, the outlier—the nonwhite, the nonmale, the nonelite—rather than ignoring them in order to focus exclusively on ideas and actions of the powerful in the public arena.

The challenge is seeing a dialectic of commonality in and through difference rather than denying it in the name of a unitary image to which all should aspire to conform, developing a dynamics of diversity that simultaneously recognizes what binds together and what separates the various segments of humanity, tracing interactions and cross-fertilizations as well as tensions and conflicts. The vision, then, is one of a new kind of shared undergraduate experience that is not only interdisciplinary and interspecialty but also intercultural—reflecting a more humane set of power relationships, an updated nonassimilationist model of cultures.

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GILBERT ALLARDYCE'S ESSAY is not simply a dispassionate description of the development of the Western Civilization course. It is instead a critique of the original conception, the objectives, and the methodology of that course. Even the staunchest defenders of Western Civ must admit that it has had its deficiencies. Allardyce's argument is convincing. With the rise of the Third World, the Western Civ course appears parochial and confining. Yet Allardyce criticizes not only Western Civ but also the concept of a required survey course in history and, indeed,

the whole idea of general education. He praises a model of a new core curriculum at Harvard because it "is neither a core curriculum not even very new, but rather a more coherent pattern of electives" (page 725). Here he goes astray. I cannot in this limited space defend "General Education," but I wish to speak in favor of an introductory survey in history.

PROFESSOR ALLARDYCE RIGHTLY OBSERVES that the Western Civ course reflected its times. It developed, in part, as a response to the two world wars. American scholars, wishing to identify themselves with the European democracies, devised a course that embodied this aspiration. They assumed that European civilization represented a higher culture whose heritage and values Americans shared. The course they developed neglected other parts of the world except when these other lands influenced Europe. Many instructors made vague references to the Arab contributions to the Renaissance or to the scramble for concessions in China or to the "brutish" Mongol invasions. Few seriously conveyed the significance of these developments in their Third World contexts. They even ignored many features of Western civilization. The course was "a history of those who were men, white, Christian, and European" (page 719). Women, peasants, Jews, Muslims (a group Allardyce himself does not mention but who played a role in European history), and children were scarcely accorded much attention. Such a course was undoubtedly limited.

Professor Allardyce's description of the origins of the course is accurate, although he is somewhat harsh on its proponents. Western Civ could be parochial and biased, and it often overlooked the world beyond Europe as well as those groups in Europe who, the historians believed, did not represent its high culture. Allardyce is perhaps overly scornful of the early proponents of Western Civ and of their limitations and biases. Like Edward Said in his denunciation of Orientalism, Allardyce is critical of scholars whose educational views were shaped by the political exigencies of their times, although he is less polemical and strident than the distinguished Columbia professor. Are not all of us, in fact, influenced by the intellectual and political currents of our era? Times and values have changed, and the originators of the Western Civ course ought not to be so blithely censured for what we now perceive as outmoded and biased approaches to the introductory survey in history.

I also cannot agree with Professor Allardyce's apparent disapproval of any required survey in history. Because the Western Civ course was limited does not mean that all mandatory introductory courses in history ought to be dispensed with. A survey of global history could, in fact, be invaluable in the college curriculum. Allardyce acknowledges that "Europe [is] no longer the world" (page 717). Education in the United States can no longer afford to ignore Asia, Africa, and Latin America. An introductory course in global history would enable the student to understand the contributions, the problems, and the perspectives of the Third World as well as of Western civilization. Such a course would seek to

integrate the histories of the various civilizations, particularly in the period after 1500. The dangers of facile comparisons and oversimplification would be ever present, but the student's exposure to the cultures and history of the Third World would compensate for the possibility of such vulgarization. I would take the risk, in hopes of expanding the student's horizons to include knowledge of Mencius, Ibn Khaldun, and the Fulani. A good introductory survey might also encourage the student to elect more advanced and specialized courses on specific areas of the world. Not only should the student's horizons be enlarged but the introductory course itself ought to incorporate a broader conception of history than the old Western Civ did. The insights of the social and behavioral sciences should supplement and underlie the traditional emphasis of intellectual history. I do not wish to suggest that the course become a trendy mixture of psychohistory and quantification, which supersedes the history of ideas. A judicious blending of both approaches is required for such a global history survey. In short, the survey course in history need not perish. It simply must have greater vision and must encompass more than European culture. There is no harm if the course reflects its own times as long as it is not beholden to specific economic and political doctrines and systems.

Professor Allardyce believes that some of the underlying assumptions and objectives of the Western Civ course *were* related to political views. He notes that "the course was a celebration of Western culture" (page 720). European civilization was equated with the advance of freedom and culture. Europe represented modernity. This self-serving, evolutionary interpretation is, of course, indefensible. Europe was not the sole center of progress. Allardyce is on solid ground here. When he disparages the goal of educating "citizens for community life" (page 696), however, his case is weaker. To be sure, education ought to emphasize the growing awareness and thoughtfulness of the individual, but a subsidiary or, some might say, equally significant objective is the promotion of responsible citizenship. Allardyce needs to explain and justify his disdain for the goal of helping to develop effective and responsible citizens. This objective ought to be one of the central concerns of a global history survey. The only caveat is that "responsible citizenship" does not simply mean preparation for a Western-oriented or Western-dominated world. A more balanced outlook would supplant the parochialism and the patriotic biases that Allardyce perceives in the traditional Western Civ course.

Professor Allardyce also criticizes one of the pedagogical objectives of the Western Civ course and of general education—namely, the unity of the curriculum. He is perturbed that the early proponents of the course used Western Civ to combat the tendencies in universities toward increased specialization. The advocates of the course espoused an "ideal of a common learning" (page 716). They believed that a common fund of knowledge for freshmen and sophomores was essential for the advanced courses. Allardyce appears to favor a system of electives with scant provision for an introductory course in history. At this time when admission to professional schools is so much on the minds even of freshmen, the need for a unified curriculum in the first two years of college and thus for a basic course in history is, it seems to me, clear. Budding engineers, doctors, lawyers, and

businessmen (and businesswomen) should have some exposure to the history of the major civilizations in the world. Students who choose to take advanced courses in history, moreover, ought to have common points of reference. Feudalism, Confucianism, and Sufism should not be unfamiliar to them. A more unified curriculum, with an introductory history course as one of its components, should be viewed as a virtue, not as a liability that constricts the student's options.

PROFESSOR ALLARDYCE'S MOST TELLING CRITICISMS concern the way the Western Civ course has been taught. Although two principal early models for the course, Harvard and Columbia, differed in their emphases and teaching techniques, both ultimately depended on graduate students or assistant professors for the bulk of instruction. At Harvard, the major professor lectured to a large crowd of students, and his teaching assistants conducted small discussion sections. At Columbia, small lecture–discussion sections were the rule. Allardyce implies that these approaches were a boon to the universities that adopted either of the models. Both types of courses were inexpensive because they relied on low-paid instructors. The professional rewards for the teachers were also minimal. Since the universities emphasized research and publication in one's own field, many instructors did not value the assignment to teach a general Western Civ course. They recognized that promotion and tenure were based on scholarship and publication often on specialized topics. Teaching the introductory course in history would not advance their careers. Western Civ frequently went, by default, to inexperienced instructors whose futures were largely unrelated to their teaching effectiveness in the course.

Professor Allardyce's arguments here are hard to refute. Western Civ often received short shrift. Some departments perceived it as a means of garnering a guaranteed enrollment that would support the more significant advanced courses. With some notable exceptions, it has been difficult to persuade senior faculty to teach the survey course in history. Without changes in the attitudes of the universities and of individual departments, no introductory course in history will be effective. A course in global history, moreover, will initially be more demanding than Western Civ. It will require retooling for many specialists in European history. Historians of the Third World will need to enlarge their concerns to include the various Western nations. To elicit such major efforts for an introductory course, university administrators and faculties must treat the global history survey seriously and must provide adequate rewards for instructors. With such changes, the introductory course can be saved and can serve as one of the cornerstones of the undergraduate curriculum.

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THE VERY TITLE OF GILBERT ALLARDYCE'S ESSAY, evoking memories of other great empires that did not quite make it to eternity, reminds us that we are in the presence of an issue of no small importance. His quotation of 1905, from Charles Homer Haskins, noting the difficulty of designing a history course for first-year college students, reminds us that there are some problems that simply will not go away. They must be confronted in every generation.

What qualifies a person to comment on an article of this sort? In my own case, it is *not* any expertise on the history of educational theory and practice; most of Allardyce's material is new to me. But my own career as a teacher of undergraduates has ensured that the issues raised in the article have been an important part of my life, and I have lived long enough to have witnessed the fall of the once proud tower of "Western Civ." As an undergraduate in the 1940s, I had the privilege of listening *enthralled*—there is no other word for it—to the superb lectures of Laurence Bradford Packard at Amherst. As an instructor at a university in the 1950s, I shared the dream of saving the unwashed masses, and the world, through the propagation of "Western Civ," and at times I almost believed I was succeeding. As an older and perhaps wiser professor at a small college in the late 1960s, I watched that dream fall apart, and I helped design our new, recommended freshman course, as a small seminar in methods of historical inquiry. Since then, my teaching has included both that methods seminar and a survey course in one period of European history (1789–1914)—but nothing so grand as "Western Civ." Every once in a while, there is a nostalgic tug back to the sweep and coherence of "Western Civ," but this has not been too difficult to resist. I suspect that, having lived through the 1960s and '70s, we can't go home again!

WITH THIS EXPERIENCE AS MY ONLY CLAIM TO ATTENTION, I would like to make two kinds of comments in response to Professor Allardyce's essay: reflections on what the Western Civ course was, and what happened to it, including a few points where I would suggest a somewhat different emphasis than that given by the author; and thoughts on the confusion and uncertainty of our own times in relation to first-year college history, and on the call by William H. McNeill, in 1976, that our profession "find something worth teaching to undergraduates *en masse*: something all educated persons should know."¹

At the beginning of *The Inspector General*, Gogol reminds his audience of the proverb that warns us "not to blame the mirror if our own mugs are crooked." In reading Professor Allardyce's insightful description of the long association of Western Civ courses with theories of general education, I found myself reacting negatively, not to the mirror but to the attitudes and educational values of those who were devising and running the Western Civ courses. In most cases, there seems to have been an undisguised arrogance in the assumption that the planners of such courses could simply mold undergraduates according to some preconceived

¹ McNeill *et al.*, "Beyond Western Civilization: Rebuilding the Survey," *History Teacher*, 10 (1977): 514.

pattern, and that the first-year students would passively accept the imprint. How rarely in his account could Allardyce point to any serious consideration by the historians giving those courses of who their students were. Yet those student generations spanned the First World War and the Vietnam War. Can we and our predecessors really have been so naive? Further indication of this attitude toward the recipients of a Western Civ education is that, despite the use of illustrative source material, or “problem books” of some sort, the pretentious sweep of the subject matter required that it be taught essentially as the teacher-historian’s *conclusions* as to what it all meant. When some of us are tempted by the coherence of the old Western Civ courses, we must remind ourselves that the coherence was usually bought at the cost of student passivity. They were, in effect, *told* what Western civilization was all about.

One must sympathize with the difficulty of selecting material for an article of this type. Professor Allardyce is surely right in emphasizing several of those schools that were pacesetters, schools whose faculties were confident in their own abilities to design programs to meet their own educational theories, and their own students’ needs, insofar as they cared to consider that. But one must also wonder what kinds of bias or distortion worked their way into the study when only such schools were considered. Did any of those faculties ever have to design a course when their college or university libraries were totally unequal to the task? One suspects not! Yet such was the fate of thousands of their counterparts throughout the country. For many of these, the single-volume, complete Western Civ textbook was not a matter of choice based on education theory but a simple matter of economic necessity. It took the paperback revolution, and postwar affluence among students in many schools, before faculties could even think about alternatives.

In one other regard I could not help but wonder what differences might have resulted from a different mix in his selection of schools. The schools Professor Allardyce considers must surely, on average, have attracted students in the top 5 or 10 percent of the national pool of undergraduates, if not always in abilities then certainly in the level of prior educational and cultural advantages. How might consideration of a quite different kind of school have altered our picture of the rise and fall of the great empire?

In discussing the fall of Western Civ, there are two points that Professor Allardyce mentions but that may deserve greater emphasis. The first is the dramatic change in the composition of teaching staffs that were expected to administer Western Civ courses. America’s coming into world awareness, during and after World War II, led to a very different mix of specialties and language competencies coming out of the graduate schools. Colleges and universities, which were hiring teachers for their undergraduates, in turn wanted to be able to offer those “exotic” specialties. It was not long before the training and interests of many of the teachers of “Western Civ” were significantly at variance with the content of the courses they were assigned to teach. A good friend of mine, now a prominent East Asian specialist, recently admitted that he took his first teaching job in a small college, contracting to teach a European survey “From the Treaty of Utrecht to the Present,” without really knowing what the Treaty of Utrecht was! Allowing for

some hyperbole, the point seems well taken. The *fall* of Western Civ was clearly related to the *rise* of our national awareness that we live in a wider, more complex world of experience. Need such an exchange be lamented?

The second point has to do with a noticeable shift in what might be called the *terms of teaching* during that critical decade of the 1960s, when most Western Civ courses died. In my own experience, two quite separate factors worked together to produce a different student-teacher relationship. One of these was improved high school training for many students in the post-Sputnik period. The fear that we were falling behind the Soviets led to upgraded courses at the secondary school level, especially in math and science. Thus the precollege education of our first-year students was given different emphasis, which stimulated new interests and priorities when these students continued into college. Beyond that, much of the improvement in secondary school math and science courses stressed that it was more important to teach the young *how to think*—within the assumptions, logic, and techniques of a given discipline—than *what to think*, at least in the form presented to them as the conclusions of those who were older and wiser. It is embarrassing to admit this today, but I suspect that a number of us, as Western Civ teachers, were simply caught in an indefensible position in the 1960s. We faced students who were beginning to think more critically, and in the process were becoming aware of the distinctions among ways of knowing. Yet we faced them with a course that had clearly sacrificed methodological sophistication for its extremely pretentious scope. Could we really engage with student questions about interpretations of the French Revolution when the course syllabus required that we gather forces and march determinedly into the Age of Metternich?

Another factor affecting the terms of teaching in the 1960s was certainly the whole mood of anti-establishment distrust that accompanied the protests against the Vietnam War. In classrooms across the nation, the respect for teachers' authority in making pronouncements, which once seemed a matter of course, was replaced by suspicion, if not hostility. Respect was not granted, it had to be earned, and Western Civ—consisting so largely of authoritative conclusions—was not the best vehicle with which to earn that respect. When argument from authority failed, the vast pretenses of Western Civ necessarily crumbled. In large part, our profession shifted to first-year courses emphasizing methodology because we had first to make the case that we were worth listening to at all.

Perhaps the greatest value of Professor Allardyce's article is that, in describing so well where our profession has been, it reminds us of where we are. And many will agree that the situation of our profession today, especially in regard to the first-year student, is far from enviable. We are fighting for turf with most of the social sciences, and, according to national statistics, we are losing ground. We have given up the old orthodoxy of Western Civ, but we have reached no agreement as to what should take its place. When we try to write prescriptions for the first-year student, we are like doctors trying to make suggestions for a patient's cure without a common understanding of what it means to be in good health. In response to this situation, what can be done?

The first and most obvious answer is to take the problem of the undergraduate

curriculum seriously and to encourage the kind of profession-wide discussion that Professor Allardyce's essay seeks to promote. Would it be so far beneath our professional dignity to have the *AHR* occasionally run a review article on how certain kinds of courses were being taught across the United States? The goal of such an article would not be to establish a new orthodoxy but simply to help us share ideas and experiences that today go no further than the walls of our own institutions. If we, as professional historians, leave these questions to professional educationalists, we will no doubt get exactly what we deserve.

For my own part, having been through the mills of both Western Civ and methodology courses, I can only offer some suggestions—not as dogmatic certainties but rather as tentative and somewhat reluctant conclusions, because I can see no other way. I am fascinated by McNeill's challenge that the only way to avoid "irrelevance and antiquarianism" is to "find something worth teaching to undergraduates *en masse*: something all educated persons should know." But I am fearful of it as well. My fear is that this appeal may evoke responses that attempt simply to define a new content of information in first-year courses—the "must know" of the educated person. Given the present diversity of fields within our profession, and given the diversity of values, attitudes, and methodologies with which we approach the past, such an agreement on content of information seems fully beyond our reach. Consensus in those terms is impossible, but it may still be possible to find significant agreement on the importance for all educated persons of some aspects of historical study that are part of our concerns as historians, regardless of our fields of specialty. Clearly, I am suggesting a "least common denominator" for consensus, but that in itself need not be considered a bad thing.

Would we not agree that a primary goal of first-year history is to engage and interest students in something other than the appalling and naive present-mindedness of our times? We may not be sure what information ought to be in the heads of good citizens, but surely we can agree that to know nothing other than one's own time and place is to be condemned to an intellectual provincialism that can serve no good end. The particular content of historical material—material used to jolt a first-year student out of comfortable, untested, cultural assumptions—is less important than the fact that the jolt be experienced at least once, and with maximum force. Considering the strength of countervailing influences on college students today, simply to help students achieve a "historical sense" is a major pedagogical victory. And, if we are honest with ourselves, that may be the most important thing most students will carry into life, if they retain any of their college history. Our ideal "active citizens" of the coming decades may know very little about the Treaty of Utrecht, but they had better be profoundly sensitive to a wide range of human experience, and they had better be aware of how traditions arise and change, and become important factors in deciding the outcome of any particular present.

Would we not also agree that historical study is an ideal vehicle for the cultivation of critical abilities that, more than information, lie at the core of an education? Obviously we share this task with other disciplines, but serious historical inquiry is especially well suited to make its contribution. Among such abilities are these: the

ability to develop authentic and self-sustaining curiosity as well as to focus that curiosity into questions that have clear meaning and are answerable; to seek out information even when it is not easily found; to use evidence in close connection with argument to reach conclusions that have claim to both truth and logic; to read critically in order to define and decide among conflicting interpretations; and, lastly, to express oneself with clarity, grace, and persuasion. No one can guarantee the full attainment of all of these very desirable goals, certainly not to a person who has simply passed through a first-year history course. They can only be gained, even in part, by an enormous effort by both students and faculty, and then not only in history classes. But I would argue that even introductory history, if properly taught, is one of the most promising places for those efforts to begin.

A final suggestion for a point of agreement among historians of different specialties and intellectual commitments concerns the questions they might raise about how human political, social, and economic systems work, change, and sometimes break down. The stress here must be on the *questions*, questions that have at least some relevance for any human community we choose to study, regardless of its time, place, or position within a particular culture. The number of such questions is far too great to list, but a selection might include questions on the order of, How is power distributed in a given polity, and what is its relationship to social and economic structures and legitimizing ideology? What is the role of individual leadership in historical change? How can one explain the origin and define the influence of ideas in a given historical context? How does a natural environment influence and in turn receive the imprint of a history that is enacted upon it? It would be naive to expect easy agreement on this or any other list, but it does not seem too much to ask members of a particular department to hammer out a reasonably coherent pattern of questions that could give first-year students some sense that they were involved in a common intellectual experience, even though they were enrolled in courses with different specialized content.

THESE COMMENTS ARE INTENDED neither as a cry of hopelessness and despair nor as an irresponsible invitation to curriculum chaos. Haskins's "most difficult question" about the first-year course is as vital today as it was in 1905. It is simply that my responses to his challenge are based on a belief that the diversities of value, training, and conviction in our profession today are so great that no new, widespread orthodoxy is likely to appear, least of all an orthodoxy like the old Western Civ, defined essentially by the information it contained. If my belief is correct, our appropriate response is not a cheap and easy "anything goes." Rather, our response should be some very serious discussions among ourselves to try to define those educational objectives that can be gained by historical study but are not dependent on a *specific* time period or tradition. I will argue that *any* course that contributes significantly to a student's interest in and sense of the past, that develops those critical and analytical skills that are so much a part of historical study, and that teaches a student to ask questions about how human communities function and change is, by those very achievements, an excellent first-year course.

And how are those excellent courses to be identified? Surely they will *not* be devised at the commanding heights of American education, to be passed along as a new orthodoxy. They will have to evolve locally in the field, in the classroom encounters of historians who understand their own training and abilities and those of their students, and who know the local situation of library resources, class size, and the amount and quality of graduate assistants. We ought to work toward a more intense and active exchange of ideas and experiences, but even that will not lead to very much uniformity. The future belongs to creative variation in first-year teaching. Is that cause for alarm?

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Reply:

A CHINESE PROVERB OBSERVES that reading prescriptions does not make one well. Yet comparing prescriptions for curricular reform is at least part of the process of restoration and renewal in universities. For most professors, this process involves imagination, conjecture—and precious little historical awareness. Authorities have described the recurring nature of curricular debates, the periodic return of the same old quarrels: compulsory requirements or elective freedom, training in depth or education in breadth, progressive experiments or “back to basics.”¹ Despite this, however, curriculum-making itself generally has been an ahistorical enterprise. “In the curriculum field,” noted one observer, “issues seem to arise *ex nihilo*; each generation is left to discover anew the persistent and perplexing questions that characterize the field.”²

Some historians have seen this missing dialogue between past and present as the cause of problems in our own discipline. Columbia University Professor Hazel W. Hertzberg, for example, argued that in the period after the Second World War, as universities flourished and the supply of students appeared inexhaustible, specialization took hold of the historical imagination, senior professors withdrew from introductory courses, and historians lost interest in teaching problems and pedagogical literature. Teachers of history, as a result, knew little about the history of teaching. When troubles came in the 1960s and 70s, they paid the price. Lacking historical perspective, historians were unable to measure the crisis in their classrooms. Reviewing their efforts in recent literature to explain declining enrollments and changing student moods toward history, Hertzberg discovered a profession in confusion; interpretations range in every direction and prescriptions provide no common remedies.³ True it is, therefore, that the question of Charles Homer Haskins in 1905 remains with us: What is to be done with freshman history?

I do not believe that the answer is in the archives. Knowledge of the past has not always brought agreement among historians, nor is it likely that more histories of old curricula and old courses will provide instructions on how to construct new ones. Historical research provides no prescriptions; it provides perspective. Thus, my work on the Western Civ course gives me little to say in response to the interesting proposals of Professors Carolyn C. Lougee and Morris Rossabi for new

¹ Laurence R. Veysey, “Stability and Experiment in the American Undergraduate Curriculum,” in Carl Kaysen, ed., *Content and Context: Essays on College Education* (New York, 1973), 59.

² Herbert M. Kliebard, “The Curriculum Field in Retrospect,” in Paul W. F. Witt, ed., *Technology and the Curriculum* (New York, 1968), 69.

³ Hertzberg, “The Teaching of History,” in Michael Kammen, ed., *The Past before Us: Contemporary Historical Writing in the United States* (Ithaca, N.Y., 1980), 489–92.

versions of the introductory course, and, in general, I would be happy to rest my case on the generous conclusion of Professor William F. Woehrlin: "Perhaps the greatest value of Professor Allardyce's article is that, in describing so well where our profession has been, it reminds us of where we are" (page 735).

BUT WHERE ARE WE? Carolyn Lougee believes that reports of the demise of Western Civ are premature. She is right. There is talk again of a need for core courses, and, by a kind of conditioned reflex, when some educators say core courses, some historians say Western Civ. A great mind once explained how men, just when confronted with a new task in their lives, fall back anxiously upon defunct ideas and borrowed language, and thus respond to new challenges in familiar old ways. Karl Marx concluded therefore that history appeared to repeat itself; now some professors appear to want to repeat Western Civ. But it is an ill wind that blows no good.

I have explained why I believe that the old, organizing ideas of this course are moribund, but I do not suggest that all Western Civ classes still walking upon the earth should turn themselves in at the morgue. Someone defined the best college course as "Mark Hopkins on one end of a log and a student on the other." Certainly a good teacher is better than the best curriculum, and Western Civ, well taught, can develop among students all of the critical abilities, communications skills, and "historical sense" that Professor Woehrlin describes as the proper goals of any first-year course. Anyway, as an old TA in the army of Western Civ, I can understand the continuing allure of the grand course. Now an aging majority in many history departments, my own generation of teachers will be the last to be raised in a world where Europe was still at the center of events, and likely some of us will continue to carry around that world in our heads—and attempt to reproduce it in our classrooms. Therefore, Western Civ is likely to be around at least as long as we are. Something like this thought probably occurred to the young academic who posted a note on his office door: "The progress of this institution . . . will be directly proportional to the death rate of the faculty."⁴

Concluding my article, I commented that some faculties, mindful of the crash of the old core curriculum of the 1960s, now were reviving general education in forms less rigid and doctrinaire. In reply, Professor Lougee announces the second coming of Western Civ at Stanford. Unfortunately, she leaves the impression that this event in 1980 provides news on the course that supersedes the conclusions that I had formed earlier from the 1976 AHA panel, "Beyond Western Civilization." Actually, this new news is old news. The course in question—called the Western Culture program—was approved in principle by the Stanford faculty in 1976 and was described at length at the AHA panel by Professor Lewis Spitz, who promoted it as a realistic, interdisciplinary effort to combine a common learning experience with

⁴ Quoted in Frederick Rudolph, *Curriculum: A History of the American Undergraduate Course of Study since 1634* (San Francisco, 1978), 17.

an elective choice.⁵ Indeed, this unique program deserves a better description than Lougee gives it: "Western Civ with only minor administrative and staffing changes" (page 727).

Students enrolled in the compulsory Western Culture program, taught in 1980–81 by faculty from ten different departments, are offered seven options, seven different course sequences, all sharing certain common readings from the great works but each otherwise concentrating on a different dimension of Western cultural development. Combined, therefore, are compulsion and choice, unity and diversity, a common core and seven separate "tracks": (1) Great Works of Western Culture, (2) European History from the Renaissance to the Present, (3) Western Thought and Literature, (4) Ideas in Western Culture, (5) Major Texts in Western Culture, (6) Western Culture and Technology, and (7) Structured Liberal Education. Some will object that coverage is limited to the European world, but other regulations require Stanford undergraduates to take at least one introductory course on a non-Western culture. The result is at the least general education in a more flexible form, a confined pluralism, something that is neither "curricular do as you please" nor "do as you are told." Just as there are recurring periods of compulsion and freedom in the history of the curriculum, so are there recurring periods in between, when wise faculties adjust core requirements to the elective principle. Someone put this in a more homely way: "More glue may be needed in the undergraduate curriculum, but it can come from several pots."⁶

None of this is to deny Professor Lougee's assertion that the world has changed since the AHA panel in 1976. Readers will notice, however, that the present *AHR Forum* is ending in the same way: four historians, four opinions, four solitudes. Similar divisions, I suspect, may arise in response to Professor Rossabi's design for a required course in global history. Universal history is, of course, the oldest form of history in the West, but few are those who would restore it to a significant place in the modern curriculum. To opponents, world history courses are simply too boundless and too superficial. To advocates, they are the verdict of common sense: citizenship in a wider world requires a knowledge of world history. The hard travail of this ideal is a history in itself. "The world war has made us internationalists in interests and relationships," wrote a historian after the First World War, when some professors attempted unsuccessfully to promote world history as an alternative to Western Civ.⁷ Their cause continues today, but it seems that historians will not be moved. "I conclude," remarked the most energetic proponent of world history, "that the historical profession . . . will have to be goaded into action from outside its own ranks."⁸ At the College of the University of Chicago, where a course in world

⁵ William H. McNeill *et al.*, "Beyond Western Civilization: Rebuilding the Survey," *History Teacher*, 10 (1977): 518–23.

⁶ Gerald Grant and David Riesman, *The Perpetual Dream: Reform and Experiment in the American College* (Chicago, 1978), 369.

⁷ Calvin O. Davis, "A Course in World History," *Historical Outlook*, 10 (1919): 451–54.

⁸ William H. McNeill, "Studying the Sweep of the Human Adventure," *Chronicle of Higher Education*, September 15, 1978, p. 32.

history is currently offered as an option to Western Civ, student support appears wanting as well. In 1978–79, for example, when 265 students enrolled in Western Civ, only 71 students chose world history and, in opinion polls, gave it only mixed reviews.⁹ Certainly no one will judge Rossabi's plea for global history on this slim evidence; but he does not have an open-and-shut case.

Professor Rossabi misunderstands me when he concludes that I scorn the fathers of Western Civ. Rather, only now, as we watch their work being undone all around us, in the high schools, the textbook industry, and the curriculum, can we comprehend the magnitude of their achievement. Nor do I think that our perception of them is brought up to date by Professor Lougee's comments on the cultural determinants of their work. She is not the first to attempt to hit them over the head with the "melting pot" thesis. This same criticism was brought against general education from the beginning, and, in the case of Western Civ itself, Amherst historian Frederic Cheyette presented virtually the same cultural analysis at the 1976 AHA panel that Lougee presents here.¹⁰ The history of education is inseparable from the history of society; therefore, inevitably, academic curricula reflect cultural values. Researchers on the curriculum have noted, however, that the forces of general education were too divided among themselves to propagate a fixed set of social norms.¹¹ In any case, specialization was the prevailing influence in the curriculum, just as individualism, innovation, and competition were the prevailing values in the nation. General education marched to a different drummer. I have explained why Western Civ was an American invention; but it was not an invention of the dominant culture. Fixed traditions, common values, and compulsory requirements were not the American dream. General education, concluded a historian of the curriculum, simply was not the nation's style.¹²

DIVERSITY PREVAILS. Therefore, many will admire Professor Lougee's proposal to develop new models of introductory history upon this principle. But already, she observes, the social sciences are out in front of us in this direction. One suspects there is a reason why. History, at best, served with difficulty as the integrating discipline of the Western Civ course; arguably, anthropology or sociology may be better suited to this role in the kind of interdisciplinary and intercultural courses that Lougee has in mind. But such issues cannot be settled by reading prescriptions. The son of Western Civ—probably there will be many—will not spring out of the heads of historians. Form and content will evolve from changing circumstances in the curriculum, the high schools, the publishing houses, college enrollments, and college budgets. This is the way it was for Western Civ; like father, like son. For

⁹ University of Chicago, *Course Evaluation: Division of the Social Sciences, 1978–79*, records of the Western Civilization program.

¹⁰ McNeill *et al.*, "Beyond Western Civilization," 534–37.

¹¹ Grant and Riesman, *The Perpetual Dream*, 376.

¹² Rudolph, *Curriculum: A History of the American Undergraduate Course of Study*, 261.

teachers of the first-year course, therefore, the way down from high history is going to be long and difficult. Meanwhile, as Professor Woehrlin says plainly, we are going to have to fight others to hold our turf in the freshman curriculum, Clio, pray for us.

GILBERT ALLARDYCE

University of New Brunswick

Reviews of Books

GENERAL

I. BERNARD COHEN. *The Newtonian Revolution: With Illustrations of the Transformation of Scientific Ideas*. New York: Cambridge University Press. 1980. Pp. xv, 404. \$37.50.

I. Bernard Cohen's present investigation—the latest in a long line of studies—is not, strictly, a work of history at all. It is rather a rich, complex, learned set of philosophical meditations on certain matters that are fundamental to any satisfactory history of knowledge, especially scientific knowledge. How do ideas grow and change? How does understanding progress? How do new concepts arise? Is there such a thing as revolution in science? It is questions of this kind that preoccupy Cohen, and his answers are best understood in three contexts.

The first of those contexts is the massive corpus of Cohen's own work. For over four decades he has been researching and writing in the history of the physical sciences in the seventeenth and eighteenth centuries (the thirty-five page bibliography included in *The Newtonian Revolution* lists three dozen publications by him). His early interest in *Franklin and Newton* and in Newtonianism led him quite naturally to the mathematics and physical science of Isaac Newton himself. In addition to articles on such subjects as Newton's concept of inertia, his use of force, his second law, and his calculus of variations, Cohen has produced an edition of the *Principia* with variant readings, has edited *Isaac Newton's Papers and Letters on Natural Philosophy*, and has labored long and triumphantly on the magisterial, coauthored *Introduction to Newton's Principia*. He also has in preparation a work on *Scientific Revolution: History of a Concept and a Name* and a series of inquiries into *Newton's Scientific Work and Its General Environment*. Against this background it is easy to understand why Cohen chose to devote his 1966 Wiles lectures at the University of Belfast to aspects of the history and historiography of Newton's scientific achievement. Those lectures were an important way station on the route to this present work.

Cohen is a leading student of Isaac Newton, but he is far from the only student, and that fact points to the second context. In the forty years since the tercentenary of Isaac Newton's birth was celebrated in 1942, Newton has been the subject of a steadily growing historical industry. The three million words of manuscript that he left to posterity have been subject to an ever closer scrutiny, and monographs have poured forth on his mechanics, his theology, his alchemy, and his history, while biographers have sought to capture the essence of the man and careful scholars have edited his correspondence and the whole corpus of his mathematical papers. Isaac Newton thus stands today not only as the supreme scientific genius but also the supremely *studied* scientific genius. In meditating on the origins, developments, and transformations of Isaac Newton's ideas, Cohen is able to draw on—and contribute further to—this vast web of Newtonian scholarship.

Finally, we should note the context of this work in the history of science. As their discipline has developed and matured, historians of science have come to understand that scientific knowledge increases neither by simple accretion nor by the opposite process of discontinuous, revolutionary change. How then to characterize those processes by which knowledge grows, and world-views are transformed? For one tentative answer—that displays both the riches of our present historical knowledge and the realities of our philosophical dilemmas—the reader should turn to Cohen's fascinating, provocative, and learned meditations.

ARNOLD THACKRAY
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JOHN EDWARD TOEWS. *Hegelianism: The Path toward Dialectical Humanism, 1805–1841*. New York: Cambridge University Press. 1980. Pp. x, 450. \$39.50.

This remarkable book is an outgrowth of John Edward Toews's 1973 doctoral dissertation; it is in fact a considerable rethinking and expansion of merely its first chapter. The ambitious task the author originally set himself was nothing less than a

systematic understanding of the entire evolution of experience and thought from the young Hegel to the radical left Hegelians of the 1840s. Thus this book is but the first part of a continuing project, one that will take us, when completed, through the transformation and disintegration of Hegelian humanism in the 1840s.

Toews's study divides into three parts. Part 1 situates Hegel's "ideology" of cultural integration in its generational context, emphasizes the role of the revolution and romanticism, and concludes by differentiating Hegel's reconciliation of reason and reality from romanticism, with which other commentators have confused it. Part 2 then examines the appropriation of the Absolute between 1805 and 1831 in the various Hegelian schools. Here the author discusses disciples and sympathizers in the early formation of Hegelianism, Hegelian politics during the restoration, and the relation between Christianity and Hegelian philosophy during the restoration, with special attention to the so-called "old" Hegelians, as well as Karl Rosenkranz, David F. Strauss, and Ludwig Feuerbach. Throughout this part of Toews's book—as indeed everywhere else—minor figures as well as major ones are attended to explicitly: for example, not only Friedrich Schelling but Franz von Baader, Henrik Steffens, Friedrich Schlegel, Friedrich Schleiermacher, Josef Görres, G. H. von Schubert, Karl Windischmann, E. M. Arndt, Hermann Suthmeyer, Christian G. Zellmann, G. A. Gabler, P. G. van Ghert, K. F. Bachmann, Christian F. Lange, C. G. W. Kastner, F. J. Schelver, Karl Daub, Johannes Schulze, Leopold Henning, Friedrich C. Förster, F. W. Carové, Henrich Leo, Gustav Asverus, Eduard Gans, Moses Moser, I. Wohlwill, K. L. Michelet, H. G. Hotho, H. T. Rötscher, K. F. Werder, Ferdinand and Agathon Benary, Johann E. Erdmann, Max Stirner, Wilhelm Vatke, and Bruno Bauer. The point of this recitation of names is to convey a sense of the enormous care and detail of this book. Part 3 then deals with the period of the division within Hegelianism (1830–40), the emergence of a distinctive Hegelian left which entailed the reduction of the Absolute to "man." Particular attention is paid to Strauss and the principle of immanence, Bauer's reduction of absolute spirit to human self-consciousness, Feuerbach's reduction of absolute spirit to human "species being." The book concludes with an epilogue on the rise and fall of left Hegelian humanism.

Together with Charles Taylor's philosophical study *Hegel*, from the same publisher, Toews's book rounds out the study of Hegel in its *historical* context as well as this has been done in several decades, and perhaps ever in English.

BERND MAGNUS
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Riverside

JOHN WILLIAM MILLER. *The Philosophy of History: With Reflections and Aphorisms*. New York: W. W. Norton. 1981. Pp. 192. \$17.95.

This work by John William Miller is a "do-it-yourself" philosophy of history: thirty-four chapters with titles like "The Static Ideal," "Accidents," "Time and Immediacy," "Order and Disorder," and "The Simultaneous and the Successive," followed by a collection of "Reflections and Aphorisms" ranging in length from a single sentence to a few short paragraphs, all in the space of less than two hundred pages. The book has no overall structure of argument: it is a scrapbook; even the individual chapters—referred to appropriately in the "Publisher's Note" as "essays"—meander down a stream of consciousness more than endeavor to make a case for anything. The reader is, in fact, knee-deep in aphorisms long before he reaches the final section: "History deals with . . . purpose as the process that revises it, not as the process that executes it" (p. 32); "There are no monuments of the past as matters of fact . . ." (p. 111); "Analogy is a form of induction" (p. 130); "Force is the will to establish the outer instruments for the possibility of reason" (p. 144)—to cite only some of the less obscure. There are certainly recurring themes: for example, the need to draw a radical contrast between history and nature, historical inquiry and science; the alleged unavailability of a presentist conception both of what historical knowledge is and what it ought to be; the relevance for any adequate understanding of history and historiography of certain ontological puzzles about time. But these are treated more as matters to be mused about than as problems to be analyzed and discussed. Among the dubious doctrines that the musing brings to the surface, and which readers are apparently expected to take on trust, are that the logic of historical explanation is "*post hoc, ergo propter hoc*" (p. 24); that understanding past actions requires the historian's discovering himself in the agent (p. 85); that R. G. Collingwood was a crude metaphysical dualist (p. 185); that the point of Leopold von Ranke's famous dictum was to distinguish history from fiction (p. 99). This is not a book on which it would be wise to rest philosophy's claim to be indispensable to good theory of historiography.

WILLIAM H. DRAY
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ERIC A. NORDLINGER. *On the Autonomy of the Democratic State*. Cambridge: Harvard University Press. 1981. Pp. viii, 239. \$20.00.

In his intellectually sophisticated and ambitious book, Eric A. Nordlinger is concerned with two central questions: how to account for the authorita-

tive actions of the democratic state, and the extent to which the democratic state is an autonomous entity. His conclusions are that the preferences of the state are at least as important as are those of social forces in accounting for political actions and nonactions, and that the democratic state is often autonomous, even when its preferences diverge from the demands of the most powerful social groups.

Nordlinger argues that most democratic theory explains public policy in terms of social pressures from those controlling the most effective resources. Most studies ignore the preferences of the state—those officials authorized to make decisions that are binding on all segments of the society—and rarely credit public officials with much independence in the face of social demands and pressures. Democratic theory thus ignores the fact that the state can often translate its policy preferences into authoritative actions.

Nordlinger briefly examines four different schools of modern empirical democratic theory—pluralist, neopluralist, social corporatist, and Marxist—and sees them all as reductionist in nature since they look to social forces for the most basic explanations of public policy. In this sweeping assessment qualification is made only for Marxist theory, which does allow that the state may act contrary to the wishes of the bourgeoisie in order to safeguard the basis of capitalist production and to maintain political stability. Equally fallacious are those modern interpretations of contemporary democratic politics that deny the autonomy of the state in speaking of the “crisis of authority.” But to say that the state is “overloaded” with too many functions to perform is not necessarily to deny its autonomy.

In his positive contribution of a complex state-centered model, Nordlinger suggests three different sets of conditions under which the state can act autonomously and notes the strategies by which the state can free itself from the constraints of social preferences and can change those preferences. The strategies depend on whether there is a divergence or nondivergence of the preferences of the state and social forces, but in all cases the state attempts to translate its preferences into public policy.

This is a highly abstract analysis at an elevated level of generality that is virtually devoid of empirical evidence for the arguments advanced. Although in general written with clarity, the book is dry and is both somewhat categorical and repetitive in its hammering home of its propositions, especially the negative assertions. Nordlinger claims that his book is in the mainstream of the realist tradition, begun by Mosca and Pareto, which criticized and revised liberal democratic theory with its argument that the political passivity of the nonelite has allowed elite dominance of politics. But his view that he has elaborated and extended this realist tradition by his

argument that the democratic state is more autonomous than even the earlier critics suggested is a rather excessive assessment of his own originality and contribution.

MICHAEL CURTIS
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New Brunswick

JEFFREY T. BERGNER. *The Origin of Formalism in Social Science*. Chicago: University of Chicago Press. 1981. Pp. xi, 162. \$16.00.

This is a slim book with a fat thesis. In something under one hundred forty pages, divided into six chapters, Jeffrey T. Bergner seeks to establish the thesis that “modern social science” is theoretically and practically bankrupt, write a synoptic history of how this condition was arrived at, and assign responsibility for this condition to the “formalism” with which the neo-Kantian school of social theory (represented by Tönnies, Simmel, and Weber above all) saddled twentieth-century social inquiry.

According to Bergner, modern social science derives not from the positivist tradition of Comte, Mill, and others, nor from the “philosophical historical” tradition of Vico, Herder, Kant, and Schiller, but rather from the critique of both of these launched by the neo-Kantians, who denied the possibility of knowledge either of the concrete particular or of the “essential nature of the whole” (pp. 74–75, 79). For Weber and the other neo-Kantians, the kind of concepts that social theory is able to generate can never “capture the essence of real entities,” for the simple reason that they did not believe that entities had essences (p. 80). For them, according to Bergner, science deals only with “what is hypothetical” (p. 81). In their analysis of the process by which concepts are generated, the neo-Kantians pushed social science in the direction of an endless reflection on “the meaning of certainty” without any hope of arriving at concepts that could actually “capture what is essential” in social or natural reality. The only kind of knowledge they envisioned was of a purely “correlational” rather than a generally “causal” kind (pp. 108–09). Hence the abandonment in modern social science of any effort to construct a “unified social science” (p. 110), the rise of a “live-and-let-live” attitude among the various departments of the social sciences (p. 111), the arbitrariness of terms like “economic,” “political,” and “sociological” in social scientific discourse (p. 112), and the nefarious “pluralism” that presides over the field as a whole (pp. 113–14). Not even sociology of knowledge, which attempted to give some order to this pluralism, provided a way of delivering social science into that condition of “maturity” that it was

always promising but never achieved. And this because sociology of knowledge was little more than a "pseudo-philosophical counterpart" of the social science it pretended to criticize. This analysis leads the author to the devastating conclusion that "the theory of knowledge which underlies modern social science leads directly to an anarchy of substantive viewpoints, each of which is immune from decisive criticism or rejection upon theoretical grounds" (p. 134). The implication—though not the argument—of this analysis is that social science theory needs some revision in the direction of a thought about "essences" and a good dose of that "philosophical history" that it abandoned under neo-Kantian tutelage.

Crucial to the argument is the thesis that the neo-Kantian tradition does indeed represent the dominant paradigm of modern social science theory, that this tradition is a purely "formalist" one, and that formalism itself is as productive of theoretical "anarchy" as the author presumes it to be. The first part of the thesis raises historicogenetic questions and would require a full-scale rehearsal of the development of modern social science theory in the period after Weber and Co. to resolve them one way or another. This is not offered in the present work. If we grant, however, for purposes of discussion, that the author is right in his presumption about the centrality of the neo-Kantian tradition, we are still faced with the problem of assessing the nature of the "formalism" ascribed to this tradition. Bergner may be right about the formalism of this tradition, but is modern physical science theory in any better condition? Is it not equally formalist on the level of its theory? To be sure, the physical sciences possess an advantage lacking in the social sciences, namely, in an experimental procedure that permits one to choose between contending theories for immediate practical purposes or technological applications. But would it be a good thing or a bad thing for the social sciences to aspire to such experimental controls? Do we really wish to be liberated from the pluralism that, according to Bergner, is the fate of the social sciences? Is it possible, one might ask, that pluralism in social theory is the very sign of the maturity that Bergner regards as its weakness?

As for the final question (namely, is formalism conducive to a pluralism that is theoretically debilitating and practically inconsequential?), this is surely open to various interpretations. It is no doubt true that in certain fields, such as linguistics and literary studies, formalism has achieved remarkable results in the transformation of what were once loosely defined "fields of study" into genuine "disciplines." It is equally true that the result of such transformations has been to produce pluralism rather than consensus in these disciplines. But the result has been less debilitating than invigorating for new

research projects, which, after all, is what theory should accomplish in the end.

The author appears to have a kind of nostalgia for what he calls "the ancient ideal of essential knowledge" (p. 137). But is this *ancient* ideal to be taken as an index of "maturity"? One might have thought that such an ideal was regressive rather than progressive. Do we wish, as the author asserts in his peroration, to "regain the meaningfulness of this ideal, and hence raise our thinking to a genuine maturity" by a "new imposition of order upon reality" (p. 137)? Who would be charged with the task of defining this "order" and who would claim the authority to "impose" it? There is a will to monism lurking behind Bergner's attack upon pluralism. In order to justify it as the order of the day for social scientists, a more richly textured argument than the one that this book provides is called for.

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STUART ANDERSON. *Race and Rapprochement: Anglo-Saxonism and Anglo-American Relations, 1895-1904*. Rutherford, N.J.: Fairleigh Dickinson University Press or Associated University Presses, London. 1981. Pp. 237. \$23.50.

Late in the nineteenth century a creed emerged that was based on a belief in the superiority of the English-speaking nations; descent from the Anglo-Saxon invaders of Britain, it was thought, resulted in innate racial characteristics that explained the primacy of English and American civilization. The intellectual origins of this movement are traced by Stuart Anderson to Charles Darwin's theories of evolution and his conviction that the lower races would be eliminated by higher civilizations. Walter Bagehot specifically applied this evolutionary hypothesis to society, but it was through the work of a minor British civil servant, Benjamin Kidd, that the ideas were popularized, influencing even Theodore Roosevelt. Historians and political scientists on both sides of the Atlantic developed analogous ideas: British and American liberal institutions had their roots in ancient Germany; the Teutonic tribes (Anglo-Saxons) transported this "institutional germ" to Britain and from there it spread to the United States. The "Teutonic origins" theory was popularized by the Oxford historians William Stubbs and Edward A. Freeman, and by the Americans Woodrow Wilson and Herbert Baxter Adams. This intellectual climate was fostered by the works of Rudyard Kipling and Jack London, both of whom translated these race theories into fiction and ensured that they had a wide audience. Anglo-Saxonism was also reflected in the visions enjoyed by

great imperialists like Joseph Chamberlain and Cecil John Rhodes. In the 1880s Chamberlain's plan for imperial federation included the United States: Americans were of "our flesh and blood" (p. 89). Rhodes's scheme for the extension of British rule throughout the world included "the ultimate recovery of the United States of America as an integral part of the British Empire" (p. 51).

Anderson argues that this common racial feeling provided the basis for the rapprochement between Britain and the United States during the years 1895–1904. Starting with the Venezuelan boundary dispute, Anderson shows the extent to which Chamberlain and Arthur Balfour appealed to "race sentiment" to ease tensions between Britain and the United States, and how, on the other side of the Atlantic, this feeling helped to turn Americans away from war in 1896. During the Spanish American War Cecil Spring Rice epitomized English thinking, which supported American expansion as "a step toward Anglo-Saxon world domination" (p. 127). During the second Anglo-Boer war official American support for Britain was largely determined by the Secretary of State, John Hay, who believed in the superior virtues of Anglo-Saxon civilization, and by Roosevelt, who based his support mainly on racial considerations. Similarly, in the Far East, Anglo-American understanding during the war of 1904–05 was deepened by the perception of a common racial enemy. Anderson argues that after this, with the intellectual undermining of Social Darwinism and the Teutonic germ theory, "Anglo-Saxonism" ceased to be an important factor in Anglo-American relations.

This is a well-researched and stimulating work. The inaccurate and anachronistic use of "racism," however, seems inappropriate. Perhaps it is a pity, too, that Anderson omitted a consideration of the ideas of Alfred Lord Milner concerning British race patriotism. The conclusion is, perhaps, not sustained by subsequent events: both Franklin Roosevelt and Winston Churchill appealed to "Anglo-Saxonism." But, overall, this constitutes a valuable study of a comparatively neglected theme.

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Aberystwyth

D. K. FIELDHOUSE. *Colonialism, 1870–1945: An Introduction*. New York: St. Martin's Press. 1981. Pp. 151. \$19.95.

D. K. Fieldhouse, the Beit Lecturer in Commonwealth History at Oxford, has provided us with a concise, learned, and judicious account of colonialism in its classic period, between 1870 and 1914.

The volume was originally intended as one of a series of short introductions to important questions and was composed on the model of articles for a large encyclopedia. There are three such articles: one each on the politics and economics of colonialism and a third dealing with its historiography. All bear the marks of an informed historical intelligence.

One cannot judge the political and economic conduct of the colonial powers, Fieldhouse cautions, by today's standard of what constitutes sound policy. The impositions imperial bureaucrats placed on subject peoples were based on models then accepted at home, he observes, and were intended to promote the welfare of the colony even at a cost to the metropolitan taxpayer. Similarly, the author displays the changing approaches of three generations of historians toward colonialism, each shaped by the major problems of the time in which the study appeared.

The seeds of the end of the empires were present at their birth, Fieldhouse suggests. What proved decisive was the growing feeling of humiliation among colonials, educated in Western schools, who had come to see their condition through the eyes of European liberals. This resentment and the desire to rule their own destinies, combined with a Western sense of guilt and a disillusionment with earlier expectations of the profits of an imperial connection, together doomed the overseas empires. Yet the imperial systems had a necessary function, Fieldhouse notes: by imposing some form of order on the expansive energies and technological intrusions of Western Europe in the late nineteenth century, they averted international anarchy; they helped, moreover, to prepare backward societies for modernization and integration into an international economy. The author does not fail to acknowledge the countercurrents of what he describes as "the dialectic of colonialism," which often justified charges of exploitation.

The chapter on the economics of colonialism is particularly useful in its treatment of tariffs and imperial monetary policy, as well as in its response to recent writings on "underdevelopment." In his efforts to disentangle writings on this issue, Fieldhouse employs three test cases—India, French West Africa, and the Belgian Congo—to good effect. Surprisingly, in dealing with the obstacles to colonial industrialization, he does not discuss the noneconomic factors that the experience of the last two decades would suggest were critical.

Fieldhouse intends his book as a reply to recent Marxist writings that explain Third World poverty as a product of colonialism. Its tone, however, is not polemical but balanced, and the study is marked by a wide-ranging erudition. Its chief defect, imposed by its form of encyclopedic abstraction, is a paucity

of illustrative examples. This difficulty can be remedied by referring to the author's other works.

BERNARD SEMMEL
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Stony Brook

SALLY GREEN. *Prehistorian: A Biography of V. Gordon Childe*. Foreword by JACK LINDSAY. Bradford-on-Avon, England: Moonraker Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1981. Pp. xxii, 200. \$22.50.

For the four decades after the heroic age of European archaeology ended around 1925, one man more than any other epitomized that study to the interested nonprofessional: the Australian-born, Marxist-oriented author of a dozen prehistorical syntheses, Vere Gordon Childe. But if any historian over forty is likely to be familiar with Childe's work, few will have much sense of its intellectual development or biographical background. This unpretentious little volume by Sally Green brings together much of what (in the apparent absence of any body of manuscripts) is likely to be available on the latter problem; if it is less satisfying on the former than Bruce Trigger's recent *Gordon Childe: Revolutions in Archaeology*, it does offer concrete historical information useful in understanding Childe's archaeological thought.

Although he did excavate one important site in the Orkneys, Childe was a somewhat indifferent "dirt archaeologist." He came to the study via training in classics at Sydney (where he read Hegel and Marx) and in classical archaeology and comparative philology at Oxford (where he established lifelong friendships with socialists who were later intellectual mainstays of the British Communist party). His archaeology was always essentially an interpretive endeavor—an attempt to understand European prehistory as a manifestation of generalized historical processes.

After publishing a disillusioned valediction to Australian politics (*How Labour Governs*) in 1924, Childe turned permanently to archaeology with several works strongly reflecting the diffusionist thinking characteristic of the 1920s—including one on *The Aryans* that the rise of Nazism made something of an embarrassment. He remained always a "moderate diffusionist," and at several points evinced discomfort with Communist orthodoxy (never actually joining the Communist party, although apparently identifying himself with it). But the most potent influence on his later archaeological interpretations was Marxian social evolutionism, which in Childe's case served to validate an often almost Victorian faith in the "progress of European civilization." At the very end of his career, the

development of carbon-14 dating made possible absolute chronologies that undercut much of the "cross-dating" by which Childe developed his syntheses. As both Green and Trigger argue, however, his thinking remains relevant even after the "New Archaeology" of the 1960s and 1970s.

An almost prototypical academic eccentric, Childe handled his retirement in strikingly idiosyncratic fashion. Depressed by the Khrushchev revelations and fearing he might become a lonely uncreative "parasite," he returned for the first time in forty years to Australia and, after visiting the sites of his youth, threw himself off a cliff near Sydney. Despite the neatly piled jacket and glasses at the top, it was only when a long-sealed explanatory letter was published in 1980 that his death was shown not to have been "accidental."

A bit free-associational, repetitive, and historically unsophisticated, the Green volume nevertheless contains much interesting material, relevant not only for understanding Childe, but also perhaps ultimately for a comparative study of influential émigré Australian intellectuals in this period (G. Elliott Smith and Elton Mayo being two other contemporary cases in point).

GEORGE W. STOCKING, JR.
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PETER C. HOFFER and N. E. H. HULL. *Murdering Mothers: Infanticide in England and New England, 1558–1803*. (New York University School of Law Series in Legal History, number 2; Linden Studies in Anglo-American Legal History.) New York: New York University Press. 1981. Pp. xxii, 211. \$22.50.

Murdering Mothers is "a provocative socio-legal study of the prosecution of women for infanticide in early modern England and New England," as its book jacket asserts. The book is the first of its kind to combine modern quantification methods and an analysis of statistics of other crimes whose numbers might be related to changes in the rate of infanticides. In addition, Peter C. Hoffer and N. E. H. Hull are careful to note the changes in the law and the interpretations given of the law by both jurors and judges. Again quoting the jacket, this study "will be of interest to both the general reader and scholar of law, history, criminal justice, women's studies and social control."

Hoffer and Hull have raised significant questions and have projected some important conclusions. Although economic circumstances appear to have had little direct relationship to the numbers of infanticides and child killings, the authors discovered that: "Those prosecuted for the murder of infants were . . . unmarried women guilty of fornication or married women known to use violence

against their children" (p. 162). In order to explain the decline of convictions for infanticides that took place in the eighteenth century, the authors cite evolving interpretations of the laws and eventually actual renovations of the laws because of altering views about childhood.

In their appendixes the authors have quantitatively analyzed their data in various ways, including the treatment of certain variables, adjustment for missing court records, and path analysis of the environmental causes of infanticide cases. There are also extensive footnotes, although some of them contain erroneous or out-of-date material. To mention one, footnote 16 in chapter 1 cites Ralph Houlbrook, *Church Courts and the People during the English Reformation, 1520–1570* (1979) on page 79. Houlbrook concluded that "church courts were losing their effectiveness as punishers of incontinence" in the early years of the reign of Elizabeth I (p. 191). In fact, that was not the case in Essex, one of the areas being analyzed by Hoffer and Hull. Throughout the reign of Elizabeth I, citations for incontinencies, pregnancies, and premarital sex crimes continued to rise in numbers in the courts of the archdeaconries of Essex and Colchester. Many so cited came when first summoned to the courts; others appeared later to clear their excommunications.

Although this book is an important publication, I must point out some corrections for Essex during the early part of the period covered. This reviewer, quite independently of Hoffer and Hull, has made a count of infanticides and child killings. Because of the lack of court records before 1560, I have done my five-year periods differently from the authors, starting in 1560. Using the eight-year-olds as the cutoff point (as Hoffer and Hull do), I have fifty-six cases of infanticide and child killing in Elizabethan Essex (taken from coroners' reports, Assize Files, and Quarter Session Rolls). The majority of women committing this crime were not married (or at least were not recognized as such in the court records), but some of them were not related to the infant or child. Their numbers reached a peak in the years 1585–89, declining by 1590 and not rising beyond the relatively low figures of 1560–64 during Elizabeth's reign, in spite of the authors' assertion that they were. These calculations, as those of the authors, should be taken with a grain of salt because of the missing records.

The authors' use of the number of murders and cases of bewitchment in conjunction with infanticides and child killings is an excellent idea. Why, however, exclude assaults from that correlation? According to my figures for Essex, if assaults as well as murders are included, the number of infanticides and child killings accounts for only 8 percent of those harmed in the reign of Elizabeth I, whereas the infanticides and child killings were 29 percent of

all those murdered (more than Hoffer's and Hull's 25 percent). Adults clearly were involved with adults most of the time. Even if the figures for so-called bewitchment of people are added to the figures of those involved in physical injuries (as seen by contemporaries), adults were much more affected. Including all cases (even those not leading to death) to 1640, infants and children constituted only 9 percent of those harmed, not "a full quarter" as cited by Hoffer and Hull for England as a whole (p. 28). If only those who died from bewitchment are counted for the same period in Essex, less than 13 percent were infants and children. It should also be noted that infant and child bewitchments were negligible after the early 1590s. In spite of the fact that the authors cite that "sixty-two per cent of all those accused of witchcraft were believed to have acted at least once against children" (p. 28), that was not the case in Essex. Only 13 percent were accused of attacking infants and children between 1560 and 1640. All witchcraft accusations in Essex declined in the early seventeenth century before the Civil War, so that there does not seem to have been much correlation with infanticides and child killings.

The authors also take into account the relationship between pregnancies, on the one hand, and infanticides and child killings on the other. They are only concerned, however, with those pregnancies reported in the county Quarter Sessions when support for the child seems to have been a major concern. They did not examine the Quarter Sessions for Colchester and Maldon. In addition to that omission, if the ecclesiastical court records for the archdeaconries of Essex and Colchester are used for the reign of Elizabeth I, many more pregnancies were cited (only about 11 percent were cited in the Quarter Sessions including Colchester and Maldon). Still others were cited for premarital sex and incontinuity in the ecclesiastical courts; others were never reported, according to the parish registers. Without a doubt, the number of children born out of wedlock was on the rise throughout the Elizabethan period. If the thirty-three mothers undesignated by marital status who were reported to have killed their infants or children are included, however, they constitute less than 2 percent of those with unlawful children. If the archdeaconry of Middlesex cases could be added (the records are in very poor and incomplete condition), the thirty-three might be as little as 1 percent for the whole reign of Elizabeth I. Actually, the number of those accused of killing infants and children seems to have had little to do with the number accused of pregnancy in the reign of Elizabeth I. That some of the pregnancies cited in the ecclesiastical courts were leading to marriage is true, but perhaps not many of them would fall into that category. A review of twenty-seven parish registers for Essex done by Lori Wolner and Lisa Capozzi revealed only 14 percent in that category. In some

respects, therefore, it is surprising that there was not more infanticide. In any case, the number of accusations was small; perhaps personal problems played more of a role than any other factor. The authors' chapter on "Individual Motivation" is well worth reading.

Much more could be said about this stimulating book, but space does not permit. Regardless of what criticism will be made of this book in the future, the authors should be congratulated for taking the study of historical crime in imaginative and sound directions.

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SHLOMO AVINERI. *The Making of Modern Zionism: The Intellectual Origins of the Jewish State*. New York: Basic Books. 1981. Pp. x, 244. \$15.50.

This book is a useful introduction and guide to the contributions of eighteen pre-Zionist and Zionist leaders to the ideological development of the movement that, in 1948, led to the establishment of the state of Israel. Chronologically, the earliest of them was Nachman Krochmal (1785–1840), the most recent, David Ben Gurion (1885–1973).

The number of important Zionist thinkers was, of course, considerably greater than eighteen, and in his preface Shlomo Avineri, anticipating the possible charge of arbitrariness, defends his exclusion of several others whom many would regard as equal in stature to those included. His argument is simple and unchallengeable: he included those whom he considered the most significant within "the incredible variety of Zionist thought."

Since Avineri's intention was to give an overview of Zionist *thought*, not of the role individual Zionist leaders played in the movement itself, there would be no point in seeking a correlation between the importance in the history of Zionism of the men discussed and the length of treatment accorded to them. The space devoted to each individual Zionist thinker, however, must have had something to do with the importance Avineri attaches to him in the evolution of Zionist thought. By this yardstick, by far the most important Zionist thinker was Jabotinsky, to whom no fewer than 28 pages are devoted, followed by Ben Gurion (19 pages), Syrkin (14 pages), then Graetz, Herzl, and Ahad Haam (13 pages each), and the others, 12 pages. Those familiar with the history of Zionism will find it difficult not to question why Herzl is played down to such an extent, since political Zionism was created by him almost singlehandedly, and he is generally recognized as the founding father of Israel both in that country and in the Zionist movement abroad.

The answer perhaps lies in the personal antipathy Avineri has for Herzl. This he demonstrates by the

many derogatory adjectives and phrases he employs in describing Herzl and his work, to wit: "almost maniacal," "sometimes superficial," "hungry for publicity," "lack of real spiritual depth," "basically lightweight," "sometimes pompous, bombastic, and theatrical," "the prophetic *chutzpah* of a beggar" (in fact, far from being a beggar, Herzl was a rich man), "internal psychological deficiencies, perhaps even the ravings of a slightly unstable soul," "very limited originality." No other Zionist figure discussed in the book is wreathed in an even remotely comparable assortment of negative accolades. It is regrettable that such a distorted picture should be given of Herzl, especially in a period in which Herzl's creation, political Zionism, has become the butt of unrestrained attacks and accusations from many quarters.

In all the chapters, and especially in the introduction and the epilogue, Avineri stresses the revolutionary aspect of Zionism. Not only does he consider Zionism "the most fundamental revolution in Jewish life," but he concludes his book on a note of hope that the difference between Israel and the Diaspora will "continue as the central facet of the permanent Zionist revolution."

All in all, and despite its anti-Herzl bias, this is a thought-provoking book, and an important contribution to the analysis of the development of Zionist ideologies.

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PAUL HOLLANDER. *Political Pilgrims: Travels of Western Intellectuals to the Soviet Union, China, and Cuba, 1928–1978*. New York: Oxford University Press. 1981. Pp. xvi, 524. \$25.00.

This is a book about Western intellectuals in the twentieth century. The topic of Hollander's discussion is a less than happy one: the often fawning affection of some Western intellectuals at various times for Soviet Russia, Castro's Cuba, and the China of Mao. Hollander is angry with this frequently embarrassing record, and his book is a journey of exploration seeking to uncover why so many intellectuals gave way to such uncritical enthusiasm.

Political Pilgrims has three strengths that taken together make it a good book. First, Hollander writes well and his tale often makes intriguing, if sad, reading. Although some readers will not find his analysis plausible, the presentation is undoubtedly stimulating. Second, it effectively weaves together disparate periods and thinkers to fashion a remarkably coherent and clear result. Third, it is a bold essay. Writing this book took guts, since it attacks so many intellectual and political parties.

Hollander is explicit about his own political sym-

pathies at the end of the book, although they come as no surprise. He offers them in the same judicious manner he employs throughout, which may make his endeavor palatable to an audience that will be diverse in its political sympathies. Moreover, Hollander always *argues* his interpretations. He uses little slapdash denunciation and depends instead on patient argument in page after page of his lengthy book.

Political Pilgrims commonly asks psychological questions: what needs and desires did uncritical faith in communist regimes provide for their believers? Hollander's view is that intellectuals looked to these experiments to vent anger over their lack of power (as they saw it) in their own nations, to express ascetic rejection of Western society, to fulfill a desire for utopianism, and much more. He often makes a good case, by intelligent speculation grounded in his sources' statements and writings, although perhaps he does not take seriously enough the realms of the intellectual and ideological as motivations in themselves. He is at his best in discussing the other side of many intellectuals' romance with Maoist China or Stalinist Russia; that is, how these nations' operatives won the praise of visiting intellectuals by playing on their vulnerabilities, above all their will to believe.

Overall his book is an almost pitiless examination of intellectual failures; perhaps it would have been fairer to spend more time on those who changed their minds, and on those, perhaps most intellectuals, who never lost their perspective and critical judgment. Hollander works hard to define what he means by intellectual, but he does not give us much guide as to how widespread the follies he recounts were and thus how to assess his deepest theme, the acute limitations of intellectuals in the West. His suspicion is real, and his book attempts to document how often many intellectuals were not rational or critical despite self-serving proclamations to the contrary.

This is an interesting book, bold in its critiques and fascinating in its documentation of the romance between (some) intellectuals and Marxist authoritarian regimes.

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ANCIENT

ROBERT MCC. ADAMS. *Heartland of Cities: Surveys of Ancient Settlement and Land Use on the Central Floodplain of the Euphrates*. Chicago: University of Chicago Press. 1981. Pp. xx, 362.

Traditionally trained historians of the ancient world who have not been aware of the "new archaeology" will be startled by this book and possibly set it aside as beyond their ken. If they do so, they will be casting themselves adrift from a major development in the study of the past. It may be difficult to understand, indeed some of it makes very heavy reading; nevertheless it is a new methodological approach that cannot be ignored.

The primary purpose of the book under review is to present the results of an archaeological survey on the central flood plain of the Euphrates in southern Mesopotamia (modern Iraq). This survey, conducted with numerous interruptions during the period 1968–75, sought to determine the pattern of ancient settlement and land use in the region and the courses of the ancient waterways. The book is divided into seven chapters containing a wealth of figures, tables, and charts.

Robert McC. Adams has a much broader goal than that of simply presenting the results of an archaeological reconnaissance. His concern is to weave together the results of this survey with the evidence of philological and historical studies in order to provide significant conclusions for understanding the development of civilization in Mesopotamia. Given this ambitious goal, it is important to be aware of the author's premise: "The archaeological evidence is not ancillary to traditional epigraphic and even historical studies but precedes them, providing an orientation for interpretive efforts rather than merely assisting in them" (p. 185).

Adams devotes an entire chapter to methodology. The first step was obtaining a supply of reliable maps of the area, but, since such maps were nonexistent, the author had to make his own. Once these maps were prepared, he and colleagues set out to explore the entire area, marking their discoveries of settlements and water courses on the maps and charts as they found them. The author then set himself the task of weaving together the results of the survey, and of what archaeology had been done in the area, with the evidence from written records in order to try and describe what was going on in this region during its long history.

One of the most important results of this study is the well-documented conclusion that given the hydrological characteristics of the two great rivers, the Tigris and Euphrates, "only the Euphrates was a significant source of irrigation water for most of Mesopotamian history" (p. 244). Of course the author is using the term "Mesopotamia" as referring to the Babylonian plain only. Another significant result is the concrete evidence for fluctuations in population over the millennia.

The author's longstanding concern with the process of urbanization comes out strongly in this book (p. 248ff.), but, while he outlines various possible

causes for the dramatic development of urban centers in the Early Dynastic I period, he still cannot provide any positive answers. Inevitably he had to come to grips with the old problem of the location of the ancient shoreline of the Persian Gulf. His conclusion: "We may have to deal in the past, as to a lesser extent we still do today, not with a well-defined shoreline, but with a progression of swamps and more and more open, more brackish or saline lagoons" (p. 16).

The author displays considerable knowledge of technical areas, such as hydrology (pp. 3–11). Although not an expert on the written sources, he takes great pains to assess the results of philological studies and to incorporate them into the results of his reconnaissance.

The reading of this book is not easy. The use of highly technical jargon, the intrusion in the text of numerous excursuses, and the constant use of statistics and reference to figures, tables, and charts make it very difficult to follow the main thread of the argument. The lack of a proper summary or conclusion is a serious deficiency. The book would have much wider appeal and be used by a considerably larger group of scholars if a short summary had been presented.

This book is to be recommended both as a comprehensive statement of an important new approach to ancient history and as the result of years of careful and methodical research following that approach. Historians of ancient Western Asia must take into account the significant results of this survey in all future research.

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MARIE-MADELEINE MACTOUX. *Douleia: Esclavage et Pratiques Discursives dans L'Athènes Classique*. (Centre de Recherches Spécialisées d'Histoire Ancienne, Équipe de Recherche Associée au CNRS, number 37.) Paris: L'Université de Besançon. 1980. Pp. 217.

According to Marie-Madeleine Mactoux "discourse" on slavery in the classical period was "multiform," not merely in its content, but in its "mechanisms of production" (p. 7). By "discourse" is meant expression considered from the point of view of its "realization," which depends in turn on the conditions of production and circumstances of communication, that is, on extra-linguistic factors (p. 10). Her purpose is to establish a lexical field of related concepts of slavery that permits reconstruction of the sociopsychological attitudes toward slavery and of the relationship of the institution to the classical polis itself. The attempt involves a complex scheme that Mactoux calls "the factorial analysis of correspondences." Mactoux is accordingly greatly con-

cerned with producing a methodology as free from bias as possible, one that can establish levels of association among different modes of discourse that intersect at multiple levels of social reality beyond the merely discursive treatment of the place of the slave in Athenian society.

There are four sections: an introduction on methodology; a lengthy analysis of the broad lexical field of associative words and concepts surrounding slavery in the Attic orators; examinations of the standard word for slave (*doulos*) in the orators, the comedies of Aristophanes, and the works of Aristotle; a brief conclusion. Of 217 pages of text, 60 are given over to charts and graphs. In general, for those who are not intimate with the jargon of the kind of sociolinguistic studies fashionable on the Continent, especially in France, the book is heavy going.

A pivotal idea is that the referential field of slavery concerns not only the attitudes toward slaves, but also involves the very notion of the community of citizens itself. The lexical field of slavery in the Athenian orators shows that their system of representation was complex. Mactoux finds there two apparently antagonistic poles. On the one hand, the slave is a (mere) possession; on the other, he affirms the civic, social, and familial existence of the possessor: "The possessed, thought of as being part of the essential goods, is necessary to the possessor to assure his being, and at the same time, the possessed exists only in regard to the possessor . . . in a word . . . the relationship is one of belongingness" (p. 41). The slave, then, is both a being possessed and a being who belongs. This duality ("possession-appurtenance") extends from the simply economic to the political realm, ensuring that, in certain respects, the slave was considered as "inseparable from the community of citizens" (p. 52). There is a further dichotomy; the *doulos* is not essentially a nonfree being, but a nonmaster and noncitizen. *Qua* a being possessed, the slave is at once included and excluded, an economic necessity both to household and polis, and a being who is shut off from all moral activity and consideration. In the concept of the slave the notions of partial inclusion and total inclusion are combined.

Mactoux finds these antinomies to be true across all the discourses studied, with certain differences. Among the orators the fundamental opposition is political—*doulos* is opposed to *polites* (citizen) and not to *eleutheros* (free). More importantly, the exclusionary notion inherent in slavery serves to endow the society with a symbolic unity. In the comedies of Aristophanes, which both create an imaginary world and reflect a historical reality, the slave also represents social affirmation. The plays justify the inequality of the slave, who is both outside the polis and necessary for its survival. A being without moral

qualities, a danger to peace and stability, yet integral to these same values, the slave becomes a very symbol of the unity of the city of free citizens, which can harmonize in itself the opposing elements. *Doulos* is thus "at the center of a complex and contradictory system, through which the notion of relative and absolute exclusion and of necessary participation is expressed, simultaneously and reciprocally. With *doulos* Aristophanes constructs the myth of a harmonious city which assimilates the slave" (p. 140).

In Aristotle, *doulos* participates in this sphere of power and counterpower under a multiplicity of forms, but the fundamental opposition is represented by the pair *doulos-thete* (the paid worker who has only his labor to sell). This most significant opposition arises from the economic, not the political, sphere, in that the economic power that is exerted over the slave is superior to that which is exerted over the *thete*, making the inherent value obtained from the slave superior. The master, according to Aristotle, is defined by the slave. Mactoux relates this to the vision of the ideal polis in Aristotle: the ideal citizen, like the ideal polis itself, is symbolized by this relationship of power and powerlessness, of belonging and separation.

I am not convinced that the notion of *doulos* can carry all the real-cum-symbolic weight that Mactoux attaches to it. Nevertheless, as an exercise in semiotics it is interesting, and many of the insights generated are provocative and illuminating.

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ROBERT J. BUCK. *A History of Boeotia*. Edmonton: University of Alberta Press. 1979. Pp. 204. \$12.50.

Boeotia was one of the larger, more fertile, and populous regions of ancient Greece. Throughout the archaic and classical periods, it was a force to be reckoned with, though it was only toward the end of this time span that it briefly realized its full potential, dominating Greece in the decade after the Battle of Leuctra. Until then, the region was usually hampered by an inability to achieve internal unity. Some aspects of its early history have attracted scholarly attention, particularly the abortive efforts to achieve unification through *synoikismos*, the constitution of the Boeotian League, and the Medism of Thebes, Boeotia's leading city, at the time of the Persian Wars. Boeotia's fourth century B.C. hegemony has been the focus of many monographs and countless articles. What have been lacking are comprehensive treatments of Boeotian history before the fourth century. The most recent survey in English prior to the appearance of the work under discussion was W. R. Roberts's *The Ancient Boeotians*, published in 1895.

Robert J. Buck's account of Boeotian affairs (excluding art, literature, and culture) from the Bronze Age to the outbreak of the Peloponnesian War in 431 B.C. goes some distance toward remedying this neglect. He begins with a survey of the physical features and political geography of Boeotia. This is a valuable reference tool, though so crammed with minutiae as to be virtually unreadable in toto. There follows a period by period survey of Bronze Age sites and a sober analysis of the literary traditions of Boeotia prior to the coming of the Boiotoi. The post-Mycenaean conquest is persuasively reconstructed through a combination of archaeological data and legend. The remainder of the work traces the evolution of Boeotian political institutions and foreign relations from the ninth century to the fifth, based on the literary record and analogy with other, better known regions. Buck's approach is marked by caution and meticulous attention to detail.

My reservations concern Buck's colorless and schematic treatment of the Boeotians. While inevitable in dealing with the earlier centuries, it continues in the later chapters, where it should have been possible to breathe more life into the narrative. For example, without attempting a full-scale cultural history, Buck might have drawn from Hesiod and Pindar to illuminate the traits and attitudes of their countrymen. In dealing with a region of many *poleis* intent on maintaining their independence, he might have made a greater effort to comprehend their individual characters, economic interests, and so forth. Though presenting a reasonable picture of the origins and evolution of the Boeotian League, he might have shed more light on the particular centripetal and centrifugal forces that operated with greater intensity there than in many other parts of the Greek world. We should be grateful for what Buck has achieved, but we still await a study that vivifies the ancient Boeotians.

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JACK CARGILL. *The Second Athenian League: Empire or Free Alliance?* Berkeley and Los Angeles: University of California Press. Pp. xvii, 215. \$24.50.

This is, in the tired adjectives of reviewing, a timely and valuable book, which deserves a place on the shelf beside the three standard works on the subject. Jack Cargill refuses simply to accept the traditional view, expressed by Busolt, Marshall, and Accame, that the fourth-century Athenian League essentially repeated the pattern of the fifth-century organization, evolving from noble beginnings into an oppressive empire. In his approach he accordingly avoids any preconceived notions about the nature of the alliance and rather than writing a chronological

history exhaustively examines the literary and epigraphical evidence.

Part 1 sets the stage with a brief survey of the King's Peace and a meticulous study of the crucial Decree of Aristoteles of 377. In part 2 Cargill examines individually every state thought to have had some sort of alliance or formal relationship with Athens during the period of the league's existence, and in part 3 he details what little is known about the organization and operation of the league. Only then does he turn to the central issue of Athenian treatment of the league members, considering in part 4 the safeguards against abuses, the evidence for cleruchies and garrisons and finally the defections from the league. The work is wrapped-up with a summary history of the league based on the conclusions reached in the earlier sections.

As you have probably already guessed, Cargill's study of the sources leads him to the conclusion that the evidence does not support the commonly accepted view of a second Athenian empire. He is able to demonstrate convincingly the weaknesses of the arguments and extremely soft nature of the evidence used to support the traditional position, making at one point a statement that should be kept in mind by everyone in the field: "The weight of scholarly opinion, however, cannot make vague sources any the less vague" (p. 37, n. 34). He is also able to provide an alternative explanation for the league defections leading to the Social War, attributing them to outside pressures and temptations rather than internal abuses.

Cargill generally makes his case. His explanations regarding rebellions in the league (which in the case of Rhodes should have considered the domestic political situation) are not entirely compelling, but they make sense, and he is certainly successful in demonstrating that the traditional view is only weakly supported and need not be true. This is not an especially exciting book, but then how many works in classical history are?

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MARC COGAN. *The Human Thing: The Speeches and Principles of Thucydides' History*. Chicago: University of Chicago Press. 1981. Pp. xvii, 309.

To Thucydides it was axiomatic (I 22) that the speeches delivered just before and during the Peloponnesian War possessed historical importance equal to the deeds that they directly or indirectly entailed, and it is through this medium that we derive a rich knowledge of the rational and emotional determinants of the actions and major phases of the war. The unique character of the speeches, unfortunately, will always perplex Thucydides' students, who are caught between admiration and

frustration by Thucydides' admixture of subjective and objective components. But if it is impossible to determine the extent to which these speeches have been shaped, edited, pointed, or altered by the historian, we may nevertheless be certain that they have not been invented: they formed an important part of the historical record and must therefore be regarded in the same light as the deeds Thucydides recorded so scrupulously.

Although Marc Cogan probably would agree with these assertions and himself insisted upon the last of them, their binding implications seem ignored in this study, where the significance of the speeches is ultimately disengaged from their historiographical context, in which they figure, first and foremost, as a primary object of historical description. The book falls into two parts, the first discussing speeches "as they figure in the initiation of events" and providing "an analysis of the interpretation of the Peloponnesian War that is structured by these speeches" (p. ix). Part 2 uses this material to isolate "the principles which informed the writing of the history" (p. ix), and Cogan finds his major principle in the singular importance Thucydides allegedly attributed to the *deliberative process*, an importance such that for Thucydides *to anthropeion* in I 22. 4 (similar wars will occur as long as *to anthropeion* remains the same) meant not "human nature" but instead "that process of deliberation which all men undertake in initiating action" (p. 237).

Only *petitio principii* can have suggested that Thucydides would have applied an expression of familiar type in this unorthodox and incredibly proleptic sense, and this curious interpretation must be rejected. The larger problem, however, concerns Cogan's extrapolation from Thucydides' speeches of his imputed interest in the deliberative process itself. Apparently, however, this has simply been assumed from the presence, and the obvious importance, of the speeches in Thucydides' historical framework, and important logical strides have been taken with the aid of a certain imprecision of language. Thus Cogan writes that "Thucydides is not interested only in demonstrating *that* the actions of the war were deliberate. He is interested as well in examining the very process of deliberation by which artificial social entities are put into motion by the individual human to whom, ultimately but not directly, the explanation of all action must be referred" (p. 197).

The deliberative process in a directly democratic society may perhaps possess the importance accorded it by Cogan. He has not shown, however, that Thucydides did more than reflect its potency in Athens, and he certainly has failed to show that it was for Thucydides that quintessential human quality guaranteeing (I 22) the occurrence of similar wars *mutatis mutandis* in the future.

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RAMSAY MACMULLEN. *Paganism in the Roman Empire*. New Haven: Yale University Press. 1981. Pp. xiii, 241. \$23.00.

Ramsay MacMullen's new book adds another to what is now a substantial series of studies by him covering themes in the social and governmental history of the Roman Empire: *Soldier and Civilian in the Later Roman Empire* (1963); *Enemies of the Roman Order* (1966); *Roman Social Relations, 50 B.C. to A.D. 284* (1974); *Roman Government's Response to Crisis A.D. 235–337* (1976). They share a number of marked characteristics, and until this latest volume one might have said that they showed a lack of development, that they remained too much on the same level. Firstly, there is the eye for a challenging and novel subject; secondly, a gift for the presentation of vivid and concrete detail, shown perhaps best of all in his justly famous article "Some Pictures of Ammianus Marcellinus" in *Art Bulletin* (1964); thirdly, an open-minded and energetic search for evidence, in no way channeled or confined by the limits of previous studies; fourthly, a freshness and rapidity of style that makes for easy reading. The product has been a highly original and illuminating series of books that have more to say than many other contemporary works, weighed down by intellectual pretentiousness or the accumulation of statistics; note his arguments on how little most "statistics" from the ancient world will tell us in "Roman Elite Motivation" (*Past and Present*, 88 [1980]: 3). The limitation his books embody has consistently been, quite simply, too great a rapidity. He is one of the few historians whose books are too short. His footnotes contain a mass of material from different sources and different areas, which cries out to be more clearly located and contrasted. Illuminating examples gain a brief allusion or characterization without ever being fully explored.

The present book, discussing the nature of imperial paganism in 137 pages of text and 69 of notes, shares all these characteristics while also representing a real advance; in my view it is his best book to date (one can safely assume that there will be more). It will not, it is true, satisfy anyone steeped in anthropological or structuralist studies of religion. He does not dwell long enough on any one community to be able to ask what role its religious ceremonies played in the functioning of society and in how people construed their world. What he does is to make a vigorous sweep over territory long dominated by the wordy generalizations of Cumont and to ask what sort of evidence we have (thinking largely of inscriptions, rather than iconographical evidence) and what sorts of conclusions we can legitimately draw from it. For instance, he shows how the supposed spread of "oriental cults" (a term that, as

he rightly says, embraces very different things, which it is misleading to lump together) partly reflects simply migration by their devotees and is partly no more than a function of the steady rise to around A.D. 200 of the total number of Greek and Latin inscriptions, followed by a decline. Similarly, the communal religious observances that marked city life in the ancient world were organized and paid for by the upper classes. If, in our evidence, pagan cults clearly seem to decline in the late third and fourth centuries, might that not simply be a change in how much the local upper classes were willing or able to pay for? Or in what they found it of use or advantage to present an inscribed record? (pp. 127–28)

The book thus offers a whole series of challenging observations as to how much we should claim to know or understand, while also insisting on the primary importance of rituals, displays, and ceremonial events rooted in particular places, and putting forward illuminating views on pagan conceptions of the divinity inherent in cult statues (p. 59), or the inappropriateness of the notion of an "official" or "state" religion in antiquity (p. 111).

This book is thus an essay on the nature and limits of our understanding of paganism as it was, in innumerable local varieties, under the Roman Empire. It is still of course far from being a full study of ancient paganism; paradoxically MacMullen's latest work represents many features of paganism in its social context more vividly and clearly than ever before, while simultaneously causing the reader to ask whether such a study ever could avoid the alternatives of being a catalogue of evidence or a series of logically indefensible generalizations, or both.

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M. GARRIDO-HORY. *Martial et l'esclavage*. (Centre de Recherches d'Histoire Ancienne, number 40; Annales Littéraires de l'Université de Besançon, number 255.) Paris: Les Belles Lettres. 1981. Pp. 241.

Martial's 1,550 epigrams were written over a space of more than twenty years (approximately A.D. 80–102). The large majority of them describes Rome and Romans during the reign of Domitian—an age of absolutism, but of high urbanity as well. Martial's deft verses might be held to embody the outlook and opinions of the Roman bourgeoisie. Toward his emperor, he is fawning; toward members of his own social class, sophisticated and cynical; and toward his inferiors, how?

The present book (originally a Besançon disserta-

tion) tackles this interesting subject in a humorless and rather doughy fashion. M. Garrido-Hory seeks "to uncover the real effects of slaveholding relations as they are expressed in the organization of Martial's poems, in their composition, in their images, and in their ideological and practical coherence" (p. 7; my translation). The earlier chapters discuss the masters, slaves, and freedmen whom Martial "names" (many are of course lay figures); the terminology of master-slave relations; the ethnic origins of slaves and the modes for acquiring them; the tasks performed by slaves; and the affective or sexual relationships between masters and slaves. Additional chapters, largely unrelated to the central theme, handle clientage and the emperor. The last chapter and a rather pompous conclusion examine Martial's views on his social system: that the free citizenry were endangered by intimate relations of freemen with slaves and by the rising numbers of freedmen.

Of all literary forms, the epigram is perhaps the one least suited to sustained observation and critique of society. The form itself presents formidable barriers to the type of grim, highly structural interpretation offered in this monograph. The author never really confronts these barriers; an introductory chapter mainly summarizes traditional philosophical views on Martial. Elsewhere the author argues, to be sure with justice, that the reality of a slaveholding system lies not far below the surface of Martial's poems: the slave is treated as an object with whom a master's affective relationship may at any moment turn proprietorial (see especially pp. 224–25). This subject invites moralization, which the author finds irresistible.

The result is page on page in which the exiguous evidence is eked out with what certainly appear to be the author's own views, not Martial's. Three poems on punishments accorded to bad cooks, and two more on ferocious razor-wielding barbers, yield the generalization that Roman slaveowners lived in constant terror of their domestic slaves (pp. 172–74). Martial's "hatred for freedmen" is extrapolated from a tiny handful of references, including the vicious "Zoilus" cycle, and even one poem expressing contempt for a censorious former pedagogue (pp. 176–79). The numerous homoerotic poems on young slaves, highly conventional poems in form and content, are contrasted in all deadly seriousness with the equally numerous poems on adulterous relations between the freeborn; Martial considered dalliance with slave boys a normal perquisite of ownership, but also favored marriage and procreation (pp. 163–68).

It is quite likely that this is the most ponderous book ever devoted to an epigrammatist. Fortunately, the author presents much of the "raw material" in the form of long tables (equaling more than 25

percent of the book), so that readers can consult Martial's text for themselves. On the other hand, a large number of mysterious graphs and flow charts serve no clear purpose.

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AUGUST STROBEL. *Das heilige Land der Montanisten: Eine religionsgeographische Untersuchung*. (Religionsgeschichtliche Versuche und Vorarbeiten, number 37.) New York: Walter de Gruyter. 1980. Pp. x, 308. \$66.00.

With its uncompromising prophets and apocalyptic tradition, Montanism represents one of the most interesting heretical movements in early Christianity. Because of its regional focus in the central highlands of western Asia Minor (Phrygia) and the wealth of epigraphic material available, Montanism has long been a focus of those who wished to study the development of early Christian movements from a geographical and archaeological point of view. August Strobel's recent work is a notable contribution in that tradition of scholarship. The author displays a thorough familiarity with the diverse sources and vast bibliography on Montanism, and his analysis is based on several field seasons in the villages and valleys of western Turkey from 1973 to 1978.

The book is described as a "religio-geographical investigation" and its primary contribution is the discussion of inscriptions and other archaeological evidence that have come to light in recent years. It is therefore a useful starting place for anyone beginning work in this area. Strobel's documentation is precise (despite the absence of an end bibliography), and he provides a good introduction to the various problems involved in the study of Montanism. In fact, the work might better have been described as "studies in Montanism," for while the treatment of individual topics is generally good, the book as a whole lacks unity and consistency and the reader certainly does not gain an easy familiarity with the "Montanist Country" (as one does, for example, for the Africa of Frend's *Donatist Church*).

The book begins with a discussion of the importance of the villages of Pepouza and Tymion as cult centers and the focus of Montanist eschatological expectation. There follows a long discussion of the location of these sites based upon the author's firsthand experience in the region. This is undoubtedly the most important section of the book, for it includes much new material, most of it epigraphic but some of a more purely archaeological nature. Unfortunately, Strobel was, like all of his predeces-

sors, unable to fix the location of Pepouza with any precision. He is convinced that the center of the Montanist movement lay in the upland valley of Kirbasan, but he cannot say more than that.

About the pre-Christian connections of Montanism, Strobel is more certain, and he accepts wholeheartedly the idea that Montanism represents the survival of earlier Anatolian religious sensibilities. Thus, he argues that the land of Phrygia itself, with its harsh environments and natural extremes, led to a particular kind of religious experience, in both pagan and Christian times. Montanism should not, therefore, be seen as a heresy that developed in Phrygia but as essential Christianity expressed from the very beginning in a Phrygian milieu. Unfortunately, this, the most significant aspect of Strobel's work, is also the least capable of proof and the reader comes away from his discussion impressed but not fully convinced.

In a work dedicated to religious geography it is unfortunate that more has not been done to help the reader appreciate fully the Phrygian landscape and the place of individual finds within it. Thus, the maps are small, difficult to find (they are not listed in the table of contents), and not very explanatory. The photographs are adequate, but of very uneven quality, and not particularly illustrative. Finally, it is unfortunate that so little attention was paid to the overall research design and the theoretical considerations that it implies. Thus, this study is decidedly "old fashioned" in its hesitance to use any newer methods of statistical or structural analysis and to tell the reader exactly how a geographical approach will help to elucidate the material. For what it attempts to do, however, Strobel's work is important and sound, and it will undoubtedly remain a basic study for some time to come.

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PAUL KERESZTES. *Constantine: A Great Christian Monarch and Apostle*. Amsterdam: J. C. Gieben. 1981. Pp. 218. f 45.

Books and studies about Constantine are legion. The question naturally arises, therefore, whether yet another one is needed. Paul Keresztes thinks so because ever since Constantine died in A.D. 337 "his memory has been maligned by ancient and modern writers" (p. 7). It is this injustice that the author wishes to remedy, by showing that Constantine was "a sincere Christian, a truly great Christian emperor and a genuine Apostle of the Christian Church" (p. 8). The author hopes to accomplish his threefold aim in 178 pages, some 77 of which are translations from Eusebius' *Historia Ecclesiastica*, the *Vita*

Constantini, and other documents, most of them easily accessible to everyone.

The first chapter deals with Constantine's arrival in Rome, his visions, and his victory at the Milvian bridge. Severe criticism is meted out to Gibbon and other scholars who have doubted the veracity of Constantine's visions of the Cross and Christ himself. In addition to Gibbon, Burckhardt, Grégoire, and others receive their share of cold showers, though Ramsay MacMullen is distinguished as a "more modern and more refined colleague" of Gibbon and Burckhardt. The second chapter corrects those scholars who "confuse" the vision "in the *Vita Constantini* with that reported by Lactantius" (p. 27). The third chapter quotes three letters of Constantine that demonstrate his support of the Christian church; then the "Edict of Milan" is discussed. The fifth chapter describes the unification of the empire under Constantine and the murder of Licinius. This chapter is called "The Licinian Intermezzo: Dissonance Removed." The sixth chapter deals with the Arians ("wolves within the fold in sheep's clothing"), the Council of Nicaea of 325, and events following. The final chapter summarizes the reasons why the author believes that Constantine was a Christian monarch and true apostle.

The author definitely has proven his first contention, namely that Constantine was a Christian long before his baptism. But the second, that Constantine was a "truly great Christian emperor" is more difficult to substantiate, and the author fails to do so. Constantine forced the old emperor Maximian to commit suicide in 310; he had his son Crispus poisoned and his wife Fausta drowned in a hot bath in 326; he killed Licinius when there was no reason to fear him any more; then he had Valens and Martinian, the two collaborators of Licinius, executed. The same fate befell the son of Licinius "and many others," according to Eutropius, 10. 6. 1-4. Can we regard these as the actions of a true Christian monarch? Perhaps we can, if we endorse the Albigensian crusade, the burning of Servetus, or the Spanish Inquisition, but even so it is preposterous to dismiss the issue with the statement: "Insistence on such arguments as these is ridiculous. The question is not whether Constantine was a 'good Christian'" (p. 172). As for the contention that Constantine was a "genuine apostle" of the Christian church, we need only remind ourselves that the Greek meaning of the word "apostle" and its ecclesiastical usage precludes its application to Constantine. By far the most objectionable statement made by the author is that Constantine's "Faith in Christianity shone brilliantly in the generosity of his material and moral support for the Holy Church of God" (p. 177). Does the granting of "limitless financial help" (p. 40) to the church cancel out murder and everything else?

Space does not permit a full accounting of the other shortcomings of the book, which range from faulty interpretations—for example, the author betrays total ignorance of the difficulties surrounding the definition of “orthodoxy” and “heresy” in early Christianity—to careless editing—misprints are numerous, for example, “will” appears instead of “with” (p. 109), “concroed” instead of “concord” (p. 123), “tells” instead of “tell” (p. 170). These shortcomings are secondary, however, in comparison with the book’s biased approach to its subject. Pope Pius XII in the encyclical *Human Generis* (1950) declared that the theologian’s task is “to show how that which is taught by the living magisterium is contained explicitly or implicitly in Scripture and in divine tradition” (*Acta Apostolicae sedis* 42 [1950]: 568 f). Keresztes has tried to write history in this spirit. Many historians will disagree with him, and even theology has come a long way since the days of Pius XII.

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MEDIEVAL

WALTER BERSCHIN. *Griechisch-lateinisches Mittelalter: Von Hieronymus zu Nikolaus von Kues*. Berne: Francke. 1980. Pp. 363. 150 FR.

This is an important book on an important subject. The topic of “Hellenism” in the West has been a perennial favorite of students of the Middle Ages since the seventeenth century. In our own times, such renowned scholars as Bernhard Bischoff, Ludwig Traube, Pierre Courcelle, Mario Esposito, M. L. W. Laistner, Albert Siegmund, and Roberto Weiss have at one point or another in their careers illuminated some aspect of it. The significance of Walter Berschin’s contribution is that his book surveys the entire Middle Ages, both geographically and chronologically, and that he brings together the results of modern scholarship as well as the considerable achievements of seventeenth-, eighteenth-, and nineteenth-century historians, paleographers, and philologists.

Berschin is not concerned with the oft-repeated question, How well did Westerners know Greek? Behind that question lurks a host of methodological and interpretive problems. What does “knowing Greek” in the context of the medieval West really mean? Although Berschin is concerned here and there with such questions, he is primarily interested in studying the influence of Greek learning, by whatever conduit and by whatever means, on Western, Latin literature. The liturgists, school masters,

pilgrims, artists, ambassadors, traders, and, above all, translators are all closely examined. Berschin’s industry and skillful reading of a wide range of disparate sources permit him to document continuous contact throughout the Middle Ages between the Greek and Latin thought worlds. Certain periods and places in Western history witnessed heightened awareness and use of Greek texts: Gothic Italy, the Carolingian realms, Ottonian Germany, Norman Sicily, thirteenth- and fourteenth-century Spain and England. Only with the catastrophe of 1453 was contact with a living Greek present broken. In its place the West erected and studied a closed, remote, “ideal” Greek antiquity. In the hands of classicists, the world of Greek learning became an intellectual museum.

In the twelve chapters that constitute his book, Berschin ranges far and wide through the world of Western scholars, schools, and translators. Latin and Greek texts (the latter printed in capitals as they appeared in medieval manuscripts) are translated into German. Particular points in the narrative are discussed in detail in bibliographical notes set in the text in reduced typeface. Detailed notes follow each chapter. As one might expect with a panoramic view of a difficult subject, Berschin has stumbled occasionally by missing or misinterpreting the findings of specialized studies, particularly of manuscripts. Such rare failings, however, are to be forgiven in a work of such scope on a major topic in medieval studies. Berschin’s book justifiably will serve for a long time as the starting point for future research on the influence of Greek learning in the West. It also has something important to contribute to medieval religious, intellectual, and educational history.

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J. J. N. PALMER *et al.* *Froissart: Historian*. Totowa, N.J.: Rowman and Littlefield or Boydell Press, Suffolk. 1981. Pp. xi, 203. \$47.50.

Jean Froissart was one of the most prolific writers of history and poetry in the late Middle Ages. His four books of chronicles still enjoy a popular audience. Froissart purposefully set about to report one of the bloodiest events in European history—the Hundred Years’ War—which resulted in one of the lengthiest and most diverse works produced in the fourteenth century. Although there are extensive biographical and literary studies about Froissart and his chronicles, no intensive treatment of him as a historian has been done.

This collection of essays by English, French, Dutch, and American authors presents a coherent, topical effort to understand Froissart as a historian. Ten essays ranging from Froissart as a diligent

researcher through Froissart as a writer of portraits to Froissart as a descriptive observer of battles present the man as one with great literary talent and imagination. As a historian, Froissart seems to have suffered in two basic respects: he rarely could go beneath surface events and analyze the institutions and persons surrounding those events, and he had a tendency to change his opinions as he changed patrons.

The coordinated essays of this volume are as follows: J. J. N. Palmer, "Book 1 (1325–1373) and its Sources"; R. Barber, "Jean Froissart and Edward the Black Prince"; J. B. Henneman, "The Age of Charles V"; J. W. Sherborne, "Charles VI and Richard II"; M. Jones, "The Breton Civil War"; P. E. Russell, "The War in Spain and Portugal"; J. Van Herwaarden, "The War in the Low Countries"; P. Tucoo-Chala, "Froissart dans le Midi Pyrénéen"; P. Contamine, "Froissart: Art Militaire, pratique et conception de la guerre"; and G. Diller, "Froissart: Patrons and Texts."

Although the authors present their studies in a clear style that will appeal to a large audience, students and scholars of historiography will be the most interested in this volume. The book contains copious notes, maps, and tables as well as an excellent up-to-date bibliography about Froissart.

DELNO C. WEST

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RICHARD KENNETH EMMERSON. *Antichrist in the Middle Ages: A Study of Medieval Apocalypticism, Art, and Literature*. Seattle: University of Washington Press. 1981. Pp. x, 366. \$19.50.

The purpose of this synthetic study is to present the orthodox image of what Richard Kenneth Emmerson calls the "medieval Antichrist" as it is reflected in prophecy, exegesis, art, and literature. Emmerson suggests that representations of this "popular view" are far more frequent and normal, at least in the early and central Middle Ages, than attempts to identify Antichrist with a particular opponent. One can hardly quarrel with this conclusion: it is confirmed by the numerous examples from art and literature that Emmerson cites. They support what Preuss called the "kirchlich-völkümliche Anschauung," the idea that Antichrist will be, in Emmerson's words, an (unidentified) "single human, a man with devilish connections who will come near the end of the world to persecute Christians and to mislead them by claiming that he is Christ" (p. 7). According to this view numerous figures foreshadow Antichrist in their malice and arrogance, but he himself is an actor only at the end of time. Emmerson regards his book as a corrective to the scholarly studies that have stressed the mille-

narian and historical element in medieval apocalypticism, emphasizing "such figures as Joachim of Fiore, Arnold of Villanova, John Wyclif, John Ball, John Huss, and other 'visionaries,' 'heretics,' 'revolutionaries,' and 'reformers'" (p. 7). Even Anselm of Havelberg seems to be considered unmedieval by Emmerson; Anselm is strangely linked by him not only with Joachim but also with John Bale and the other Protestant reformers who equated the whole Roman church with the Antichrist.

Emmerson has produced a useful and erudite corrective, but his approach involves certain sacrifices. It is schematic rather than chronological and therefore reveals little appreciation of Antichrist's incursions into history, which were memorialized, for instance, in pamphlet wars between papal and imperial supporters in the thirteenth century. Such propaganda could have been the subject of an interesting chapter. Emmerson also says nothing about intriguing peripheral subjects like the relationship between the mystical and open antichrists or the connections made between Antichrist and the reception of Aristotle. He is not interested in the transmutations of Antichrist's image but only in its original form.

Slips are inevitable in a book of such wide scope. Michael of Cesena (p. 69) was never Olivi's supporter. It was seceders from the Franciscan Community like Michael and William of Ockham and not primarily Spirituals (p. 206) who attacked papalist claims. The bibliography has odd gaps: for example, it contains no mention of Raoul Manselli or H. M. Schaller. But despite minor blemishes the book is a valuable introduction to a vast and important subject.

One blemish, caused not by the author but by the publisher, should not escape castigation: seventy pages of footnotes divided according to chapters are lumped together at the end of the book, and at the top of the pages, repeated seventy times, is only the word "Notes." No thought has been given to the convenience of the reader, who must in consequence leaf back and forth every time he wishes to match a text reference with a footnote. Obviously it is late in time, and the malice of publishers appears to be ominously waxing.

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EDWARD J. KEALEY. *Medieval Medicus: A Social History of Anglo-Norman Medicine*. Baltimore: Johns Hopkins University Press. 1981. Pp. x, 211. \$16.50.

It is a welcome exception when our knowledge of early European medicine does not depend solely on academic sources but is enriched from records such as the charters and chronicles that were painstaking-

ly combed by Edward J. Kealey. His book is not only more fully documented than the most recent histories of medicine in medieval England by C. H. Talbot and by Stanley Rubin, but it is also tighter in thematic and chronological scope. The author focuses on the expansion of health care between 1100 and 1154, an era that he first explored in a study published in 1972 on Roger of Salisbury. He seems more successful, however, in widening the web of known contacts among twelfth-century people than in presenting either a genuine social history of medicine or a coherent characterization of the *medicus* in the Anglo-Norman world.

The introductory chapter, breezily titled "Miracles, Marvels, and Monarchs," sets the tone of a narrative unhampered by logical exactitude: some procedures were "quite advanced for the time," the naturalists were "intolerant of traditional Western concepts," and physicians were "professional practitioners." The last-mentioned anachronism contradicts both the era's admittedly "fluid classifications" and the author's broad interpretation of the term "physician." This interpretation allows him to assemble (in chap. 2) individuals as varied as faith healers and phlebotomists together with all those surnamed *medicus*, and to profile (in chap. 3) eight royal attendants whose medical careers appear rather incidental. However useful, this assimilation does not warrant the attribution of a "corporate existence" to the assemblage. Moreover, such comprehensive categorizations, applied further in the survey of institutions (chap. 4), do not yield criteria of the availability of health care; nor can they provide the premise for ratios such as "roughly one physician for every 2000 people" and "at least one hospital bed for every 600 to 1000 persons," let alone a basis for comparison with modern statistics. Finally, estimates of availability need to be correlated with some idea of utilization and actual practice.

To the student who, heeding John of Salisbury's plea, looks beyond logic, *Medieval Medicus* offers a rich mosaic of humanity and refreshing insights into health concerns. The solid chapter on charitable foundations finds their source in aristocratic and civic patronage more often than in ecclesiastical initiative—a revision borne out in French regional studies. In the last chapter, hospital regulations are vividly illuminated in the rule of the Dudston leprosarium. Two valuable appendixes contain a directory of ninety "physicians," which substantially supplements the *Biographical Register* of Talbot and Hammond, and a list of 113 "hospitals," which is essentially drawn from existing catalogues. The full bibliography and endnotes attest to the author's extensive research.

There are far more misprints than one expects from a distinguished press. If most of these may be blamed on gremlins, quite a few misspellings seem

due to hasty editing (gullability, exhorbitant, caracuate, cemetary, *Cisternensis* and Cistericans). Particularly disturbing are the errors that suggest a lack of diligence with medical terms (*tempermantum*, *Epidimale*, symptom); some of the latter may have the effect of bad puns (climactic zones, venerally, sporific), but they can only distract the reader, weaken the author's effectiveness, and cloud the real merits of this fascinating study.

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H. E. HALLAM. *Rural England, 1066–1348*. (Fontana History of England.) Atlantic Highlands, N.J.: Humanities Press or Harvester Press, Sussex. 1981. Pp. 309. \$27.50.

Since the publication of H. S. Bennett's *Life on the English Manor* in 1937, scholars have come to appreciate the fact that any one English manor differed profoundly from most others, and numerous local studies of rural life in the distinctively varying geographical regions of England have been the result of that realization. *Rural England, 1066–1348* is an attempt to digest the wealth of information that has resulted from these studies, and H. E. Hallam suggests perspectives on rural conditions that differ significantly from those offered recently by scholars such as J. Z. Titow, M. M. Postan, and R. H. Hilton.

The eight central chapters survey the local studies made in each geographical region of England. Each chapter then discusses five or six themes, allowing comparisons among the regions and demonstrating a large degree of variation from one manor to the next, even within a single county. The themes include population growth and new settlement, crops and livestock, agricultural techniques, peasant resources, and inheritance customs.

Hallam attempts to set his minutely detailed descriptions of manorial economy into the broader context of Western cultural history. He begins in the first chapter far afield from his actual data by arguing that the ideology of the Latin church formed the basis for materialism and individualism and thus provided the spirit of capitalism. He suggests that because the Protestant work ethic was not new to the Reformation, expanding cultivation and increased productivity during the period from 1066 to 1348 cannot be properly understood apart from the history of Western Christian ideology.

In his final chapter, Hallam summarizes his own observations and contrasts them with the picture of Malthusian crisis proposed by Postan. Hallam suggests that the English population rose continuously between 1086 and 1294, reaching a peak of from five to seven million in the latter date, but he argues that agricultural improvements (including the use of

horse power, the cultivation of nitrogenous legumes, and advances in field systems and crop rotation as well as the use of various forms of crop nutrition) made it possible for England to support this large population and that famine and food shortages were not evident even in the heavily populated eastern counties. He points to recent evidence of geographic mobility among the peasants indicating that this was "no closed or subsistence economy" (p. 254), and he argues that the peasant's diet was heartier and healthier than has been assumed.

These discussions are provocative and are a good indication of the direction of current research in the field and of the many problems remaining to be investigated. It is unfortunate, however, that Hallam takes little advantage of the social history that is now emerging from village court roll evidence and still relies most heavily on studies based on manorial estate records. In spite of the title of this book, the social history of the rural village as community and center of peasant economic and political activity is barely mentioned. Determining whether a region exhibited evidence of partible or impartible inheritance customs is as close as Hallam comes to discussing the social history of the village as opened up by men such as George Caspar Homans, a scholar he praises so highly. Certainly it is to the peasant villager we should turn in our search for the motivations and nature of economic and demographic expansion rather than to Jerome, Augustine, the Penitential of Theodore, or even Langland.

One final observation: Hallam's exploitation of the methodology of the historical geographer is much appreciated, and he clearly demonstrates the interrelationships between local ecology and the cropping and livestocking decisions made by landlords. It is, however, unfortunate that not a single map accompanies the text.

ANNE REIBER DEWINDT
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RALPH A. GRIFFITHS. *The Reign of King Henry VI: The Exercise of Royal Authority, 1422–1461*. Berkeley and Los Angeles: University of California Press. 1981. Pp. xxiv, 968. \$35.00.

This is a truly massive book, weighing 900 grams, with 992 pages and over 3,600 footnotes. The bibliography alone covers 29 pages and includes the titles of no less than 61 unpublished theses and dissertations in addition to manuscripts and published primary and secondary sources, all showing a formidable knowledge both of traditional scholarship and the recent spate of work in fifteenth-century English studies, which make it impossible any longer to regard this era as the neglected

century of English history. The volume's erudition is as monumental as its appearance, and one can only envy Ralph A. Griffiths's obviously phenomenal memory and phenomenal notes. Griffiths now provides what has so long been sadly lacking: a sound narrative of his chosen period, a narrative at the same time more detailed and more cautious than that of Bertram Wolffe's recent *Henry VI* (1981).

Griffiths knows the early and mid-fifteenth century like a well-informed gossip columnist and places every political development firmly in its immediate setting of personalities and that all-important cement of the contemporary scene, patronage. He thus avoids the jejune and misleading compression that has always been the bane of narratives of this period. The author illuminates almost every subject he touches. He praises the much misunderstood minority council of 1422 as "a capable, experienced and vigorous group of men." He gives by far the best accounts so far written of the political crisis of 1426–27 and of Cade's rebellion. He superbly depicts the atmosphere of squalid disintegration that developed during the 1440s as Henry VI allowed himself to be more and more dominated by a narrow self-seeking court faction at home and as the situation in Normandy went from bad to worse owing partly to the alarming poverty of military talent in the English high command. The account of Richard of York is balanced and judicious, showing the provocation that he received from Suffolk and Margaret of Anjou while seeing him as no more than a mediocre military commander at best and as a somewhat naive and tactless politician, possibly more dependant than most magnates on a group of councilors, who may have pushed him into somewhat extreme courses. In his brief periods of power York behaved more like a self-interested magnate than a genuine reformer, and he was surrounded by fellow magnates like Salisbury, Warwick, and the third Mowbray duke of Norfolk who were hardly less rapacious and corrupt than the detested Suffolk had been before his murder in 1450. From 1455 onward Henry VI sank into ever-deepening inertia, perhaps even a kind of torpor, leaving the direction of affairs more and more in the hands of his formidable wife. Griffiths also maintains that Henry and his advisers anticipated Yorkist methods of chamber finance in their administration of the lands of the attainted after 1459. Such are but a few of the many topics on which the author presents both sound judgments and a good deal of new information.

After Griffiths has given us so much it may seem ungracious to move onto a carping note. Just as Spenser's *Faerie Queene* is a poet's poem, this is a historian's history. There is hardly a chapter of it that the specialist will not read with great profit, and

I look forward with considerable pleasure to lying on a sofa rereading and mulling over various sections of the volume. The undergraduate and the general reader will, however, be better served by Wolffe's book. For them Griffiths will often be too allusive. For example, it is difficult to grasp from his brief account the disastrous folly of Humphrey of Gloucester's personal ambitions in Hainault, and the significance of the Parliament of 1453 could be much more clearly demonstrated. The narrative of the years between 1456 and 1460 (always cursorily treated in earlier works) is by far the best account that has so far appeared, but it gives the impression of being unduly rushed and compressed in comparison with earlier parts of the book. Much more could be made, for example, of the intrigues and ambitions of the legate, Francesco Coppini.

The bibliography would have benefited from greater care and a more thoughtful and consistent arrangement. In its immensity there seems to be a certain degree of scraping the barrel for minor items, yet such important works as M. G. A. Vale's book *English Gascony 1399-1453* and M. R. Powicke's article "Lancastrian Captains" are absent. Confusingly, some primary printed sources are listed under the names of their authors, others under the names of their editors. The Roxburghe Club edition of "The Boke of Noblesse" is listed twice, once under the name of the author, William Worcestre, once under that of the editor, J. G. Nichols, with two different publication dates for what is obviously the same edition. But these are small blemishes in a book that combines such vast erudition and professional expertise.

J. R. LANDER
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SUZANNE FONAY WEMPLE. *Women in Frankish Society: Marriage and the Cloister, 500 to 900*. (The Middle Ages.) Philadelphia: University of Pennsylvania Press. 1981. Pp. xviii, 348. \$22.50.

The centuries between the collapse of Roman authority in the West and the creation of a new society have appropriately been called "dark" because the sources of their history are at best opaque. It is often difficult to obtain even an elementary, not to mention consistent, impression of the barbarian peoples who came to dominate the European continent on the heels of imperial disintegration. Until new questions were posed and innovative methodologies designed to attack problems inherent in periods of history where poverty of evidence is endemic, the history of women remained obscure and seldom went beyond a catalogue of pious or shameful behavior of prominent women. Students of the early Middle Ages have waited for a comprehensive

treatment of the history of early medieval women, and, although Suzanne Fonay Wemple's *Women in Frankish Society: Marriage and the Cloister, 500 to 900* does not mark the end of that long wait, it is a very welcome addition to the growing corpus of scholarly literature that assesses women's position and the forces at work in expanding and limiting their sphere of activity and scope of influence. This illuminating study, based in part on the author's earlier works on the position of secular and religious women in early European society, presents a thorough synthesis of scholarly endeavor of the last few decades and of relevant contemporary sources.

In assessing women's function and status in Merovingian and Carolingian society, Wemple examines the growth of secular and ecclesiastical institutions and concomitant social stratification, and she focuses—though by no means exclusively—on the shift from polygamous to monogamous marital practices among the higher echelon of early Frankish society and the emergence of the nuclear family under the impact of Carolingian legislation and pressure from the male-dominated Frankish clergy. Roman and Germanic cultural amalgamation effected an increase in female rights and the expansion of women's roles, and conversion to Christianity was initially a liberating factor in the lives of Frankish women. These early advantages disappeared, however, as the rudimentary social, political, and religious structures developed into more sophisticated institutions. Sexual inequalities were intensified by placing greater emphasis on female private concerns of nurturing and housekeeping, which consequently restricted noblewomen's power and influence and limited their opportunities and roles. Furthermore, increasingly sexist conciliar legislation, which reached a peak in the reigns of Charlemagne's successors, isolated religious women from the mainstream of Frankish life through demands for clerical celibacy and women's exclusion from the church hierarchy. Wemple's study clearly shows that the position of women in Frankish society was not static and that it differed in each social group and fluctuated within each group under the impact of various historical pressures.

The author of this rather short monograph could have been less anecdotal—the subject is complex and some readers will want certain statements qualified by more detailed analysis—but the text is sound, the prose is eminently readable, and the points of criticism that could be raised are minor ones in the context of the accomplishments of this book. The inclusion of an excellent bibliography, informative notes, helpful genealogies, and comprehensible tables detailing conditions of women's land tenure evinced in a few monastic cartularies is certainly laudable. Wemple has written an intelligent and interesting book about the transformation

of Frankish society from a relatively primitive tribal structure to a more complex hierarchical organization under the Carolingians and has provided valuable insights into women's experiences in a crucial and formative period in Western civilization.

MARC ANTHONY MEYER
University of Rochester

FRANZ J. FELTEN. *Äbte und Laienäbte im Frankenreich: Studien zum Verhältnis von Staat und Kirche im früheren Mittelalter*. (Monographien zur Geschichte des Mittelalters, number 20.) Stuttgart: Anton Hiersemann. 1980. Pp. 368. DM 198.

Earlier historians—such as Sackur, Voight, Lesne, and Amann—used to attach singular importance to the lay abbot during the secularization of the Frankish-Germanic church in the ninth and tenth centuries. The work of recent historians, however, has shown that the social and political roles of the monastery were very complex. Schlesinger, Semmler, and Prinz have examined the links among lay abbots, aristocratic nobles, and monarchs. In this excellent book Franz J. Felten continues in this scholarly tradition. Although little in this work is original, it is the most thorough and systematic treatment of the lay monastery to date.

In his survey of royal policy toward the monasteries from the sixth century through Louis the Pious, Felten carefully assesses abbatial behavior in terms of the attitudes of nobles, bishops, and the people at any given time. The image of the abbot changed, as did the notion of monastic reform. Felten draws from a wide variety of chronicles and conciliar, capitular, and hagiographical sources, as well as from modern studies of particular monasteries. The first half of the eighth century was crucial in the evolution of the abbatial rank within the church hierarchy. During the reigns of Pepin and Charlemagne the place of the abbot was fixed within the ecclesiastical order. Felten greatly refines the older commonplace that the ninth-century lay abbot was a greedy noble who exploited his monastery. The most penetrating sections of the book deal with the transition of the role of the abbot at the time of Charles Martel and Pepin and with Louis the Pious's continuation of his father's policy of immunities. Felten focuses on the ambiguity of the abbot that resulted from Charlemagne's privileges, Louis the Pious's reforms, and the changed functions of the episcopacy.

What makes Felten's study different from previous treatments of the abbatial office is the extended parallel analysis of bishop and abbot. To put the social function of the monastery in historical perspective, the abbey is juxtaposed to the corresponding role of the bishop in the ecclesiastical and royal

structures. The resulting balance serves to illustrate the limits and possibilities of the lay-controlled monasteries in the ninth century.

THOMAS RENNA
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DANUTA POPPE. *Économie et société d'un bourg provençal au XIV^e siècle: Reillanne en Haute Provence*. In collaboration with GEORGES DUBY. Summary in Polish. (Académie Polonaise des Sciences, Institut d'Histoire de la Culture Matérielle.) Wrocław: Ossolineum. 1980. Pp. 265. 90 Zł.

This study of Reillanne, a Provençal town of some twenty-four hundred persons in the early fourteenth century before the plague halved its population, is a welcome addition to urban literature. Small towns have been sadly neglected in favor of larger cities and their more abundant sources, but Danuta Poppe demonstrates that painstaking research in notarial registers can produce a vivid account of small town life.

Reillanne's economy could be described in two words: agricultural and local. Agriculture, including a substantial investment in livestock, was the mainstay of the little community and directly or indirectly provided a living for most inhabitants. Both the town's industry (notably, leather goods and a small cloth industry) and its commerce were based on agricultural products and were oriented toward local consumption. Although local tradesmen did import cloth (mainly from Languedoc, less from northern France and the Low Countries), long distance trade played a minimal role in the town's economy. Inventories of household goods suggest that imported luxury goods would not have found a ready market in Reillanne. The richer citizens preferred investments in land to conspicuous consumption. Partly because the town had so little to offer besides agricultural products and by-products, Reillanne played only a modest role in the region's economy. In competition with a town in a more favorable location and another with a popular fair, Reillanne could do no better than a poor third.

Thanks to the plague, Poppe argues, Reillanne's inhabitants enjoyed a higher standard of living during the latter half of the century. A decline in rents and an increase in the amount of land available on favorable terms helped the small farmer, while the artisan enjoyed higher wages. The substitution of wheat for cheaper grains in diet, a decline in consumer debt, and an increase in household goods as the survivors consolidated their inheritances all testify to an improvement in the quality of life. Corresponding changes in the social structure did not, however, occur. Poppe found that the same families dominated municipal offices throughout

the century and weathered the crisis of the mid-century with their social and economic position intact. Such families characteristically combined several interests: they dealt in grain or cattle, engaged in trades such as shoemaking, and, like everyone else in Reillanne, invested in land whenever possible.

Poppe has mined her sources for every scrap of information and argued her case effectively. Nonetheless, given current interest in popular piety at the time of the Black Death, some comment on pious donations might have been appropriate. In addition, this study exhibits the failing of many pioneering ventures: it cries out for comparative material. This defect can hardly be blamed on Poppe, however, and will be remedied when others have followed her lead into the archives of small towns.

PATRICIA LEWIS
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FRANÇOISE AUTRAND. *Naissance d'un grand corps de l'état: Les gens du Parlement de Paris, 1345-1454*. (Publications de la Sorbonne, NS Recherche, number 46.) Paris: Publications de la Sorbonne. 1981. Pp. 459.

This book seeks "to show how an institution and its personnel were transformed into a *grand corps de l'état*" (p. 14). Based on a computer-structured inquiry into the careers of 678 *présidents*, *conseillers*, and *gens du roi* who served in the Parlement from 1345 to 1454, it is a social study that moves beyond the older histories of Parlement to establish a more convincing chronology of institutional change. The court reorganized in 1345 was still a concentration of notables identified by status as clerks, nobles, masters, and others—"a microcosm of French political society" (p. 266). By the end of the fourteenth century their social character had become more closely connected with their function, itself more professional, and their selection points to the increasing influence of family or alliance. Before 1418 some 70 percent of the members were related to other *parlementaires*, a proportion said to be over twice that of 1345. These and other factors—juridical culture, privilege, longevity of tenure, and a proud self-consciousness in the king's service—lead to the conclusion that Parlement had come to form a distinctive, socioprofessional milieu about 1380. From then until 1418, when it was jolted and dispersed by princely conflict, the institution attained its apogee of prestige and influence. Reunited in the middle of the fifteenth century, Parlement began to lose its autonomy as the concept of proprietary office caught on. To the age of the allies succeeded that of the heirs.

The book is most successful in characterizing the

newly incorporate Parlement under Charles VI. In extended discussions carried mercifully beyond the categories of mechanical analysis and correlation that prevail in the early chapters, Françoise Autrand shows how the incipient *noblesse de robe* assumed power in Parlement and how the older nobility reacted. There is some mention of ceremonial life. The author is no slave to the computer, and it is worth the effort to study her tables to grasp the interest of her demographic questions and to appreciate the solidity of her quantified results. Nevertheless, the periods with which she begins and ends are less well served by her methods. Too much is made to rest on the ordinance of 1345 to admit of an altogether secure profile of early Valois parliamentary society. Even for the generation best represented in the *catalogue prosopographique*, one would like to know more about jurisprudence, scribal culture, and the influences that played on Parlement. The unrelievedly prosopographical stress makes this a difficult book to read, and the decision to separate the text from the notes and tables does not help. But these limitations hardly affect the considerable achievement of this book. The developed Parlement was among the great institutions of early modern France. The origins of its peculiar social and professional texture needed explaining and that need is well met here. Moreover, the author's discriminating resort to systematic data processing may be suggestive to historians in other fields interested in working from prosopographical data to historical chronology.

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BRUCE E. GELSINGER. *Icelandic Enterprise: Commerce and Economy in the Middle Ages*. Columbia: University of South Carolina Press. 1981. Pp. xix, 299. \$19.50.

Icelandic traders covered distances in dangerous seas with simple equipment that far surpassed anything produced elsewhere in medieval Europe. This book is a summary of Iceland's commercial history from the first years of settlement in the late ninth century to annexation by the Norwegian crown in 1264. An epilogue takes the story to about 1500. In both range and detail the book deserves to be called definitive. The sources, principally Icelandic sagas and histories, are used with an understanding of the problems of reliability. The book includes extensive background on the Icelandic constitution as well as excellent concise summaries on ships and navigation, so that it is valuable both to new and old hands.

The major portion of the book is in the two parts devoted to Icelandic foreign trade, treated first geographically and then chronologically. This divi-

sion unfortunately yields some repetition. From about 930 to 1022, when the king of Norway made concessions to Icelanders, the pattern of trade was established. The island sent woolen goods, especially low-quality cloth, to Norway in exchange for grain and some timber. Similar exchanges existed with other partners, but those trades were dwarfed by direct contact with what was Iceland's mother country. From about 1022 Icelanders became passive, leaving trading to Norwegian shippers. The rising population made land values go up and labor costs go down, so that investment in land seemed a better option than risking life and property in an open boat on a lengthy voyage. Production expanded at home and exports increased, while imports supplied basic needs. From the late twelfth century, foreign markets were less able to absorb Iceland's products and could not fulfill its demand for grain. Norway played on the Icelanders' plight, interrupting trade at times and finally forcing them to accept Continental rule.

The pattern of economic change fits with that in the rest of Europe, but there the rising incomes of landowners in the twelfth and thirteenth centuries meant more rather than less trade. Bruce E. Gelsinger finds the explanation to some degree in the size of Iceland's productive capacity and in that of its principal trading partner, Norway. The author mentions climate sparingly. The very few attempts to quantify are tantalizing in their rarity. Also missing is information on the rules that governed the trading enterprises. The whole question of the balance of payments, a constant source of trouble for Iceland and a contributing factor to changing domestic prices, is not given enough consideration. Icelanders finally overcame their chronic deficit in the late thirteenth century by replacing woolen cloth with dried cod as their major export good. It is surprising that they did not stumble on that solution earlier. Despite any such criticism Gelsinger, by describing the commercial history of Iceland in the framework of Europe's economy, succeeds in showing that internal population growth and declining export markets were major reasons for the final destruction of the Icelandic commonwealth.

RICHARD W. UNGER
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A. S. KHOROSHEV. *Tserkov' v sotsial'no-politicheskoi sisteme novgorodskoi feodal'noi respubliki* [The Church in the Social and Political System of the Novgorod Feudal Republic]. Moscow: Moscow University Press. 1980. Pp. 223. 2 r. 20 k.

No one has done more to construct a data base combining archaeological and historical sources and a coherent model of life in medieval Russia than

V. L. Ianin in his work on Novgorod. Ianin is the editor of A. S. Khoroshev's book on the Novgorod church and, not surprisingly, Khoroshev relies on Ianin's scheme of Novgorod politics. After the usual historiographical introduction, Khoroshev recounts chronologically the history of the church. The book's final chapters on the church's structure, land ownership, and role in Novgorod governance and economic life are more interesting and original.

Khoroshev demonstrates that on every level the church and the boyar aristocracy intertwined to dominate the city-republic's existence in proportion to their ability to exclude the jurisdiction of grand princes and metropolitans. The town council first elected its hierarch in 1156. Thenceforth, boyars controlled most selections. About 1250 archbishops became head of the republic; they led its diplomatic efforts, administered the state treasury and lands, and attained widespread judicial authority. Boyar families by 1417 endowed and controlled six of seven main churches, each associated with a particular end of the city. Each administered other churches of its district and was a center of commercial activity. Most of the city's monasteries were of the "founder" (*kititor*) type and similarly controlled by boyars. Khoroshev's major contribution is his refinement of data on church landowning, drawn mainly from surviving cadastre books (*pistovye knigi*) developed by A. M. Gnevushev before 1917 and by Soviet scholars under A. L. Shapiro. Moscow compiled the first cadastre immediately after subjugating Novgorod. Khoroshev rightly insists that only its "old register" is a reliable guide to landowning patterns under the republic. In three appendixes Khoroshev lists each tract separately and gives its size, number of households and taxable peasants, location, and type of owner (archbishop, monastery, church). The archbishopric held most of its land in several large plots, lending credence to notions that these were state lands that it administered. Khoroshev's results also sustain arguments by L. V. Danilova and S. A. Tarakanova-Belkina, based on smaller samples, that a few boyar-controlled "founder" monasteries were, with one exception, the biggest landowners.

In Ianin's model, boyars of each end fought incessantly for control of the city; their quarrels were primarily responsible for Novgorod's turbulent history. Khoroshev agrees in describing the maneuverings of the archbishops in Novgorod's politics, but it is evidently obligatory that he also remark on the causal role of class conflict (although the scanty evidence rarely allows a critical assessment of its significance) and trot out the four horsemen of traditional Soviet historiography on Novgorod—B. D. Grekov, B. A. Rybakov, L. V. Cherepnin, and V. N. Bernadskii—as references. Khoroshev asserts (again, on slender evidence) that

the *Strigol'nik* heresy was an antifeudal and free-thinking mass movement. He sides with N. A. Kazakova that Archbishop Vasilii tolerated and perhaps sympathized with the "popular ideology" in the 1350s, and that he was neither Palamite (A. I. Klibanov) nor heretic (Rybakov). For all that, the book is a solid and interesting contribution to the history of medieval Europe.

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MODERN EUROPE

DAVID GRIGG. *Population Growth and Agrarian Change: An Historical Perspective*. (Cambridge Geographical Studies, number 13.) New York: Cambridge University Press. 1980. Pp. xi, 340. Cloth \$47.50, paper \$17.95.

MICHAEL W. FLINN. *The European Demographic System, 1500–1820*. (Johns Hopkins Symposia in Comparative History.) Baltimore: Johns Hopkins University Press. 1981. Pp. 175. \$15.00.

The current interest in demography and demographic history appears to operate on two planes. On the one is the analysis of human behavior, largely through the examination of parish registers of baptisms, marriages, and burials and the reconstitution of families. It is a narrow field, and work in it is slow and laborious. Nevertheless the practice of family reconstitution, first pursued in France in the 1950s and then in Great Britain by the Cambridge Group for the History of Population and Social Structure, has yielded important results that have great value both for social history and for current demographic problems. On the other plane is the broader study of aggregate populations, their size and growth, their migration, and their relationship to available resources.

These two books reflect respectively the two levels on which the subject is at present being studied. They are not, however, mutually exclusive. Both concern themselves with the cycles of population growth and stagnation as manifested in Western Europe, and neither can avoid discussing the crises that have at intervals decimated the Western population. But by and large they are complementary, the one a survey of the results achieved so far by the microstudy of the behavior of small communities, the other a macrostudy of the growth of world population and of the utilization of the resources by which it is supported.

Michael W. Flinn's study of European population between 1500 and 1820, roughly the period for which parish registers are available, is the shorter

and more incisive work. It begins with a survey of research on the reconstitution of families done over the past thirty years from the data presented in the registers. Most relates to Great Britain, France, Germany, and Scandinavia, but some work has also been done on parishes in Belgium, Spain, and Switzerland. Much of the resulting data, including that on fertility, birth intervals, illegitimacy, premarital conceptions, age of women at first marriage, and infant mortality, is summarized in a statistical appendix that occupies a quarter of the book. For the rest, Flinn examines the trends in fertility, mortality, and migration before analyzing the changes that led to the breakdown of the preindustrial demographic system. Although highly condensed, this is a clear and balanced presentation. It addresses itself to most of the major questions raised in demographic history: the influence of epidemics and of *crises de subsistence*, the importance of age at marriage, the crude attempts to control family size, and the influence of partible inheritance and domestic crafts. On most of these topics the author formulates carefully considered conclusions. He argues that, since early population growth had been held in check "by recurring short-run crises rather than by a sustained high level of mortality . . . some reduction in the frequency, severity and ubiquity of these crises could have accounted for [the] lessening of the grip of the positive (i.e. Malthusian) check." A factor that deserves but does not gain consideration is age-specific mortality during these crises.

If there is a weakness in this sensibly argued and perceptive book, it is its failure to look at the resources necessary to support a preindustrial population. There are repeated hints that at times the pressure of population on food supply was dangerously high. It certainly became catastrophic during the crises that periodically (as about 1600, in the 1690s, and in 1816–17) interrupted the slow growth of population. Perhaps this is how the positive check worked. There was never a real food scarcity in good harvest years, and the view that population growth can be shown as a curve asymptotic to a line representing a steadily available food supply is clearly untenable.

It is this latter problem, the recurring cycles of population growth and stagnation seen against a food supply that itself fluctuated in response to climatic and technological change, that engages the attention of David Grigg. It is a wide-ranging study, in which the argument is sometimes overwhelmed by the sheer volume of the evidence. Its starting point is the Malthusian concept of overpopulation. It then defines optimum population and proceeds to an analysis of the long-term trends in the demographic history of Europe. Three cycles of growth are defined, the first two culminating about 1300 and 1600 respectively, the third initiated in the mid-

eighteenth century and still maintaining its upward course. The question is asked—and answered—were the halts to growth occasioned by Malthusian pressure? The answer in the cases of 1300 and 1600 is yes. In the former case Grigg is probably right, but not in the second.

In the third part of the book, "Malthus Refuted," the author turns to instances of population growth that did not encounter a positive check: the Netherlands in the sixteenth and seventeenth centuries; England, France, and Scandinavia in the eighteenth and nineteenth centuries. The explanation lay in part in the technological revolution in European agriculture—although the author is careful not to place this too early—in part in the creation of a world system in which the satisfaction of local demands for foodstuffs could be satisfied at the expense of other and sometimes distant regions. The Dutch were thus able to break through their demographic barrier because they could import grain from the Baltic.

The concluding part of this book examines population growth in the Third World, "which greatly exceeds that in nineteenth century Europe," and the expansion of the food supply, which does not. The author emphasizes that the history of Europe's population holds few lessons for the rest of the world. Emigration is no longer practicable on a significant scale, and the system can no longer be enlarged to embrace new sources of food and materials. In short, there is no longer a frontier. Hence the title to part four: "Malthus Returns?"; the guarded conclusion is that he does.

This is in many ways a helpful book. If the broad pattern of population change is familiar, the more intimate studies of particular countries are likely to be particularly useful, bringing together as they do a vast amount of disparate material on agriculture and population. The vast body of source material used has, perhaps inevitably, introduced a number of errors, most of them of minor importance. The truly valuable English hearth tax (p. 89) was that of 1664; the cession of the *Reichsland* to Germany was in 1871 (p. 190). There are several misspellings of place names; Hurepoix (p. 104) is a *pays* name, and the Earl of Upper Ossory (p. 124) is not known to *The Complete Peerage*.

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HARTMUT LEHMANN. *Das Zeitalter des Absolutismus: Gottesgnadentum und Kriegsnot*. (Christentum und Gesellschaft, number 9.) Stuttgart: W. Kohlhammer. 1980. Pp. 186. DM 44.

Hartmut Lehmann's book is volume 9 in a fourteen-volume series, entitled "Christentum und Gesell-

schaft," that surveys the whole Christian era. Intended more, perhaps, for the general reader than the specialist, the series places church history in a broad context of related cultural and political developments. Its perspective is not narrowly confessional, but it aims at a synthesis of the histories of many different Christian groups and movements.

Lehmann does a fine job of bringing order to a chaotic period in Christian history. His task is to deal with an expanded seventeenth century—viewed as a transition from the Reformation to the Enlightenment. It would be difficult to make any one, significant generalization about all the varieties of Christian phenomena that flourished in this age of crisis. Lehmann points out that Christians of one kind or another can be found on most sides of all the major issues. Some eagerly cooperated with and supported the evolution of absolutist governments, others found themselves in conflict with the state. Some pressed for conformity to carefully defined orthodoxies, others favored new freedoms for individual consciences. Some expressed their faith intellectually by adherence to doctrines, others emphasized piety and charity. In the first half of his book Lehmann lays out the church-state situation in Spain, the Netherlands, Austria, Bohemia, Hungary, Germany, Scandinavia, France, England, and North America. He has cogent explanations for the abundance of different ecclesiastical policies that faith and military necessity forced on the governments of Europe.

The more original portion of the book is its second half. Here, under the heading "Not, Angst, Hoffnung," Lehmann deals with the intellectual and emotional crises that troubled the seventeenth century. He discusses the significance of the era's enthusiasm for edifying literature and has much to say about the popularity of radical Christian beliefs and the pervasiveness of superstition. The period showed a preoccupation with prophecy, eschatology, chiliasm, and magic. The sense of emergency or pending dissolution that hung over the age shaped its ethical thinking. Lehmann depicts the seventeenth century as a time of labor toward uncertain goals, which finally ushered in the birth of the more secure and confident world of the Age of Reason.

The publisher of the series, W. Kohlhammer, has not done well by its authors. The choice of minute type—for large pages with narrow margins—and the use of an imperfectly opaque paper make the text something less than a delight to read.

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G. J. MARCUS. *The Conquest of the North Atlantic*. New York: Oxford University Press. 1980. Pp. xiv, 224. \$25.00.

This book by G. J. Marcus is a concise and scholarly review of what is known of the maritime activity of Western Europe in the North Atlantic from early Christian times down to the late fifteenth century. The field is, in practice, limited to the waters west and northwest of Ireland, Great Britain, and Scandinavia, and focuses primarily on Irish and Norse shipping, maritime techniques, and activity at sea in relation to Iceland and Greenland, with valuable material on English and Hanseatic contacts with Iceland in the later Middle Ages. The work is very well documented by primary printed sources (and some English manuscripts) and by secondary literature in a number of European languages. It is necessarily brief on a number of extensive and sometimes controversial topics, but its judgments are carefully arrived at and are in general cautious and well conceived. It puts before the reader much technical work from Scandinavian sources on the navigation techniques that brought the Norsemen into northern and western waters.

The book will find some critics on particular topics. Irish skin boats, for example, are expertly discussed, but the use of wooden vessels by the Irish in early times (now maintained by serious scholars of Old Irish) is ignored. Similarly, Marcus's conclusion that Pytheas did not get beyond the Faeroes was vigorously contested at a conference on the early history of the Arctic at Rome in October 1981. The work's most valuable feature is that it ties together the expansionism of the Scandinavian lands westward, indicating that the urge toward the Faeroes, Iceland, and ultimately Greenland (and, briefly, North America) was that of pastoralists who balanced fishing with stockbreeding and who also came to exploit the valuable seal and walrus resources of the northern lands, both for usable protein and for valuable exports such as walrus ivory.

The outline of the history of Iceland and Greenland, from the aspect of maritime history, is well done. Marcus's view that Bjarni Herjólfsson, rather than Leif Ericsson should be regarded as the first discoverer of America will now be accepted by many. But his approach to Leif Ericsson's achievement is somewhat vitiated by failure to keep up with the most recent publications. He rightly chastises Helge Instad and S. E. Morison for premature assumptions that the L'Anse aux Meadows Norse site must be Leifsbudir, but Marcus is on unsure ground with later work. He ascribes the noncontroversial report on the excavations by Anne Stine Instad (*Acta Archaeologica* [1970]) to her husband and wholly ignores her fine book, *The Discovery of a Norse Settlement in America: Excavations at L'Anse aux Meadows, Newfoundland, 1961–1968* (1977), which objectively describes what was found. Although we have yet to see Helge Instad's interpretation of this material, objective evidence makes it difficult to believe that there was another major Norse site in

North America, unless the sagas conceal more than they reveal, especially since Marcus stresses that the Greenland colony was too weak in personnel and shipping to maintain a western outpost for long.

The decline of Scandinavia and the subjection and subsequent desertion of Iceland by Norway in the late thirteenth and fourteenth centuries are well brought out. Marcus gives the appearance of the English as fishermen and traders new importance in the maintenance of the Icelandic economy until Hanseatic intervention in the late fifteenth century seriously curbed English activity for a time.

The book is in many respects a stimulating and revealing one that makes available to specialists and nonspecialists alike, in manageable form, much research and investigation that has not hitherto been easily accessible, if at all, to English-reading scholars. Over a wide area, traversed with considerable skill, Marcus's judgments are in general well sustained, so that his book will provide students of Atlantic history with an essential starting point for the exploration of what has previously, in some branches at least, been a highly recondite field. As such, the book is to be warmly welcomed.

DAVID B. QUINN

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MARGARET C. JACOB. *The Radical Enlightenment: Pantheists, Freemasons, and Republicans*. (Early Modern Europe Today, number 3.) Boston: George Allen and Unwin. 1981. Pp. xiii, 312. \$25.00.

This admirably presented monograph is the third in the "Early Modern Europe Today" series, which is designed to offer fresh material and new viewpoints in its field. The format is conventional except for placement of the bibliographical essay before the text—a device less significant in itself than as a handy preview and source of later numbered references. (The two systems of citation by numbers—the second referring to chapter notes—are very clearly differentiated typographically.) The book's editing has been exemplary; also excellent are the bibliographical essay, the index, the appendix (of proto-Masonic and early Masonic documents), and above all the precise writing, careful researching, and fascinating substance of the text itself. The whole, it should be noted, is offered within the context of modest intentions and recognized limitations: Margaret C. Jacob presents no blockbusting reassessment of the Enlightenment and its sources, yet succeeds all the more impressively in suggesting new insights into what may well be more than a mere footnote to eighteenth-century history.

Her central argument is that one interesting road to the High Enlightenment has been little known and its significance much underestimated: the movement of pantheism, materialism, and republi-

canism that originated (though with Renaissance precedents) among radicals of the English Revolution and the new Newtonian science, and flourished between 1690 and 1750 in Masonic circles in the northern Netherlands. These circles included English and Dutch politicians and writers, and especially French Huguenot publishers of advanced books. Jacob's research and documentation are painstaking and sometimes pathfinding, notably in original sources in the shadowy realms of early English-French-Dutch freemasonry. The author is careful to differentiate radical pantheistic, naturalistic, and republican strains from the dominant strains of mild deism and moderate monarchical theory that ultimately were developed by Montesquieu and Voltaire. The reader is introduced to such radicals as John Toland; Prosper Marchand, Jean Rousset de Missy, and their essentially Masonic, quaintly named Knights of Jubilation; and the ubiquitous publisher of Enlightenment extremists, Marc-Michel Rey. Also included is that notorious landmark in clandestine religious and political radicalism, the *Traité des trois imposteurs*, for which, Jacob argues, the main responsibility lay with Rousset de Missy.

The author also re-creates in scholarly, vivid manner the social world of the Radical Enlightenment, with its salons and coffeehouses in The Hague, its literary and publishing enterprises, and its aura of embattled reformism and vaguely mystical aspirations. Finally she presents several rather tenuous links between the Dutch centers of early eighteenth-century radicalism and the High Enlightenment in France, embodied notably in Denis Diderot's great *Encyclopédie* and the Baron d'Holbach's anticlerical materialism. Jacob's discussion of both is brief—hardly more than a postscript—but judicious. The *Encyclopédie*, she rightly insists, was hardly a Masonic project, as is sometimes alleged, but it did have one Masonic author (the Abbé Yvon) and another (the Chevalier de Jaucourt) with close Masonic connections in Holland. I found this work a stimulating study.

HENRY VYVERBERG
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DICK GEARY. *European Labour Protest, 1848–1939*. New York: St. Martin's Press. 1981. Pp. 195. \$25.00.

Although offering few new or surprising interpretations, Dick Geary presents an admirably brief and persuasive overview of European labor protest from the early days of industrialization to the outbreak of the European portion of World War II. He delineates three phases of development—emergence (up to 1890), maturation and organization (1890–1914), and organizational splitting (1918–39). Within this chronological framework, Geary argues that three

basic types of protest—preindustrial, early industrial, and modern industrial—frequently coexisted, but, as the names imply, in general he sees the types as being related to the chronology. A multinational approach strengthens the work, although occasionally the author must strain somewhat to get, for example, Spanish and Russian events to fit patterns based on West European and English models. Geary's command of a broad range of secondary sources is impressively demonstrated by the nearly four hundred seventy-five footnotes that accompany the fewer than one hundred fifty pages of text.

One of the main themes of Geary's interpretation is that "resort to political action was often a function of industrial impotence" (p. 94), which is convincing for the English and German examples, though considerably less so for the French. Perhaps the strongest section of the book is that which deals with the question of the presumed deradicalization of the working-class movement in the years immediately prior to World War I; it is the best argument against this once-popular notion I have ever read. Geary is not nearly so effective, however, when he tries to reinterpret the significance of the war itself as a radicalizing force; the war remains, I believe, the central event in the history of the working-class movement of modern Europe.

For such a short work there is a surprisingly large amount of nearly verbatim repetition, especially in the sections dealing with Germany (pp. 91 and 113; 62–63 and 117–18; 112 and 119). In addition, Geary's protestation against studies of the working-class movement that are really institutional or ideological histories is somewhat outdated; for at least the past decade, American and European scholars alike have recognized the importance of the distinction between such works and examination of rank-and-file activities.

This is a very complex topic, and though nearly anyone could quibble with Geary's specific interpretations, he has performed a valuable service by drawing together a large amount of material and providing useful generalizations. A rewarding course could be developed around this book using regional and national variations to test hypotheses; scholars working on particular national movements are well advised to read it in order to provide themselves with an international perspective. Geary's book should prove a stimulus to more synthetic and original work in the field of labor history.

GARY P. STEENSON
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R. J. CRAMPTON. *The Hollow Detente: Anglo-German Relations in the Balkans, 1911–1914*. Atlantic Highlands, N.J.: Humanities Press or George Prior, London. 1981. Pp. 255. \$18.75.

R. J. Crampton demonstrates that a detente (in its formal sense) undoubtedly existed between Berlin and London in their handling of the Balkan crises in the years 1911 to 1913. He shows, further, that the Germans were inclined to hope that it would turn into a permanent and lasting improvement of relations, to the detriment of Britain's ententes with France and Russia. This aspiration belonged from the beginning to the realm that Lichnowsky, in July 1914, would call pious hopes.

Crampton usefully elaborates the course of German-British relations and the brief collaboration of these two powers. He performs a scarcely less useful service in explaining the intricacies of the Albanian problem. Most diplomatic historians have contented themselves with saying that it *was* a problem, and (understandably) few have wished to go into details. Crampton sets forth the extraordinary ethnic and religious distinctions and tensions, the arcane conflicts between Ghegs and Tosks, the relations of both to the Turks, the ghost of the Venetian empire hovering, a potential threat, over the city of Scutari, the position of the Greeks in regard to Janina, the explosive confusions (still explosive in 1981) in Kossovo, and the aspirations of all neighboring governments.

The picture that emerges from all this is of a Germany and a Britain that regarded the Balkans as a nuisance, although scrupulously supervising their own interests in the eastern Mediterranean and Constantinople. But there was never any question that both remained unshakably attached to the interests of the diplomatic partnership to which each belonged.

The book is soundly and widely based on what seem to be all the accessible documents, published and unpublished, from German, British, French, and Austrian archives. It includes—a comparative rarity among the writings of Westerners—consideration of many Bulgarian sources. The scholarship is towering, and although some readers may regret not hearing more about Russian or Serb policy, those are unfortunately spheres in which historians cannot enter with the freedom that the archives of the Western governments afford. On the complexities of the Balkans themselves, and especially on the motives and reactions and opinions of responsible British and German authorities, the study is entirely satisfying. More detailed studies may appear, but it seems unlikely that any will cause major revision in Crampton's narrative and conclusions.

LAURENCE LAFORE
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Tony Ashworth's book is about those human realities of war that operate to limit, mitigate, and de-escalate the violence that is proper to the "idea" of war as defined by Clausewitz. The dominance of defense in the First World War, a dominance that was enhanced by the technological acquisitions of the prior fifty years, was such as to render insupportable those norms of offensive behavior that were sponsored by the administrators of the war. From 1915 on, among troops engaged in combat on the western front, there arose what soldiers at the front called the "live and let live system," which "denoted a process of reciprocal exchange among antagonists, where each diminished the other's risk of death, discomfort and injury by a deliberate restriction of aggressive activity . . ." (p. 19). The ritualized violence, significant inactivity, the routinization of aggression constituted tacit, always illicit truces that covered specific aspects of trench life and governed the actual work of war. "Live and let live" was the ethos of the masses in war, of Tommies, *poilus*, and *Frontschwein*, and Ashworth's description of the details, nuances, and language of this system is the most valuable part of the book.

In his analysis of the response of the British high command to the "inertia" of the troops, Ashworth advances beyond his 1968 article in the *British Journal of Sociology*. As units were absorbed in the routines of trench warfare and their alienation from the army staffs increased, the staffs reacted by insisting upon fire ascendancy over no man's land, instituting a policy of aggressive patrolling, promoting "thrusters," and regularly shifting troops between active and inactive sectors. But more significantly in terms of the new industrial realities of war, they specialized weapons groups. In 1916 heavy mortars and Lewis guns were removed from battalion provenance to become a part of divisional organizations. Special assault groups were formed as permanent units in the German and Italian armies (the *Stosstruppen* and *Arditi*) for specific missions. In this case specialization made men more violent, for it removed specialists from the society in which the norms of aggression were collectively governed.

The themes and materials of Ashworth's book should be revealing to those who continue to take an "official" view of war, seeing it in terms of the ways in which leaderships push each other away from their plans. But the book has its analytical shortcomings. The limitations on violence that arose from day-to-day life in the trenches are treated as a "system" rather than a "cycle," and thus the dynamics of combat are de-emphasized. Ashworth's analysis is based exclusively on British materials and he can offer no comparative perspective. It is impossible to learn about other fronts or other wars or to determine the extent to which the limitations on violence characteristic of war on the western front

TONY ASHWORTH. *Trench Warfare, 1914–1918: The Live and Let Live System*. New York: Holmes and Meier. 1980. Pp. xi, 266.

were produced by moral, social, and technological forces unique to that time and place. Most significantly, Ashworth tends to regard the restraints on aggression as a product of a collective mentality, the offensive norms as something imposed on the troops from above. He thus neglects those phenomena of war that reveal the extent to which these motives were in conflict *within* individual combatants. This conflict was often intense enough to produce the numerous instances of psychological breakdown that one finds among combatants.

ERIC J. LEED

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MARTIN GILBERT. *Auschwitz and the Allies*. London: Michael Joseph or Rainbird, London. 1981. Pp. xii, 368. £12.00.

As one of Britain's most prominent contemporary historians, Martin Gilbert has written not only his multivolume biography of Winston Churchill but also a number of other books dealing with the fate of European Jewry during the tragic years of the Second World War. In taking up this controversial theme, he uses his masterly knowledge of the documentary sources, principally British, to argue his case with lucidity and fluency of style. His method may be called cinematographic; that is, he juxtaposes short narrative passages to show what is happening on various parts of his canvas, so that the reader is made aware of the total scene in a series of skillfully mounted montages in short chapters, with rapid changes of actors and scenery.

In *Auschwitz and the Allies*, Gilbert's thesis is that the persecuted Jews of Europe were neglected by the Allies, the reports of their victimization and murder were ignored, and appeals for assistance were consistently turned down. The reasons for this, Gilbert believes, can be attributed in part to the successful secrecy of the Germans and in part to the reluctance of the Allies, particularly Britain, to alter their set policies to take account of these atrocities. At the beginning of the war, the lack of accurate information about what had happened to the innumerable Jews deported to unknown destinations was linked to the practical impossibility of assisting them. But by 1944, Gilbert claims, much more could have been done, including such measures as the bombing of Auschwitz or the railway lines approaching it or the opening up of havens of refuge, particularly Palestine. Gilbert's conclusion is that the Allies failed to take advantage of these opportunities because of the lack "of imagination, of response, of intelligence, of piecing together and evaluating what was known, of co-ordination, of initiative, and even at times of sympathy."

This thesis is not new. Two recent books pub-

lished in 1979 and 1980 (Bernard Wasserstein, *Britain and the Jews of Europe, 1939-1945* and Walter Laqueur, *The Terrible Secret*), have covered the same ground and used many of the same sources. Gilbert's narrative approach adds depictions of events not just in London but also in Washington, Geneva, Jerusalem, and occupied Europe, including survivors' memories from Auschwitz itself. He thereby contrasts the unfeeling decisions taken by the Allied policy makers with scenes from the victims' points of view, as the Auschwitz inmates waited in vain for some sign that anyone on the Allied side cared or as the bedraggled refugees who managed to escape from the Nazi slaughterhouse were heartlessly denied entry to the refuge they sought among their fellow Jews in Palestine.

Gilbert's main contribution is to provide, in the latter part of the book, a full description of the way in which the first authentic eyewitness accounts of Auschwitz were brought to the West in early 1944 by four escapees who made their way to Slovakia, where they sought to alert the Jewish authorities to the impending fate of the Hungarian Jews, whose deportation began shortly after. In fact, these warnings were published in the West only two months later, a development that Gilbert skillfully covers over. At the end of June 1944 Churchill himself was shown the evidence and at once pressed for strong measures, including bombing raids. Churchill was the one man who understood the enormity of these atrocities, Gilbert states, in contrast to the lack of comprehension and imagination that characterized not only many of the Allied bureaucrats but also many Jews themselves. But in the making of Allied policy, "Churchill was not always the final arbiter, and in many cases, not least among them the bombing of Auschwitz, other considerations prevailed."

Gilbert successfully shows up the prevarications of the British Foreign Office and Air Ministry and the American War Department, who argued that bombing raids on Auschwitz would "risk valuable lives," even at a time when equally dangerous missions were being flown to neighboring areas of Poland to bomb industrial targets or to drop supplies to the beleaguered Resistance fighters in Warsaw. Auschwitz had indeed been photographed from the air on a number of occasions and once was even damaged by bombs dropped there by mistake. The gas chambers and crematoria, however, were never touched.

Gilbert follows Wasserstein in laying the blame for this negative Allied response on the failure to make the saving of Jewish lives a top priority in the war effort. He is less critical of the British refusal to take up the incredible Gestapo offer to exchange a million Hungarian Jews for ten thousand trucks to be used on the eastern front, if only because in this instance his hero, Churchill, unequivocally forbade

any dealings with the Nazis. He is also careful not to attribute the Allied insensitivity to outright antisemitic prejudice, even though he can quote some examples. Instead, he builds his case on the continued British fears of measures that would strengthen the Jewish presence in Palestine or the desire not to single out any one group of Nazi victims for preferential treatment. By juxtaposing the details of these attitudes with the evidence of the Nazi successes in murdering Jews in Auschwitz and other concentration camps, Gilbert makes a powerful case.

But Gilbert's own failure to consider other aspects of the military and strategic situation makes his case one-sided. He gives hardly any space to the counter-arguments that placed higher priority on the need to marshal available resources in the theaters of war where victory could be achieved first. He admits that even the Jewish Agency in London came to the conclusion that the bombing of Auschwitz would do little good to the inmates and quotes one Air Ministry source that rightly pointed to the impossibility of mobilizing sufficient strength at such a distance, when all available aircraft were needed for the invasion of France. He also admits that the only effective way of saving the Jews would have been to reveal the Germans' plans but glosses over the Jewish leaders' failure to warn their fellow Jews of their imminent fate. Gilbert's attempt to shift the responsibility by blaming the Allies for not supporting more positive rescue measures ignores the fact that, as early as 1942, these Jewish leaders in Europe were aware of the Nazi extermination policy but did little or nothing to discourage the passivity of those who were destined for "resettlement." Thus a lack of balanced analysis makes this book an argued *plaidoyer* that does not provide us with final answers but will serve to stir up controversy over this fateful and deadliest chapter in recent Jewish history.

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FRANJO TUDJMAN. *Nationalism in Contemporary Europe*. (East European Monographs, number 76.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1981. Pp. 293. \$21.50.

Franjo Tadjman is a former general in the Yugoslav army who fell from grace in the 1960s and since has been arrested twice for participation in Croatian national causes. His book might be called an interpretation of modern Europe from the Croatian point of view. There is much that is salutary in this, particularly in Tadjman's insistence that nationalism is a great humanistic principle. Confronted as we are today with so many examples of fanaticism in

the name of nationalism, it is easy to forget that the positive underlying idea of the nationalist is the right of every person and every people to determine their own fate.

Tudjman is also correct, I think, in his assertion that since self-consciousness has arrived for good among all ethnic groups, "the solution of the national problem is essential for the construction of a . . . stable European order" (p. 4). The efforts of currently constituted Western European states to construct the European Economic Community on the basis of respect and reciprocity provide an excellent example of how this might be accomplished, although Tudjman does not dwell on this example.

Instead, Tudjman is more interested in Eastern Europe. For example, he provides a sound discussion of how the Great Russians are accorded a leading role in the Soviet Union. But his most interesting comments concern Yugoslavia. As an unabashed proponent of the Croatian position, Tudjman makes some points that need airing. For example, sometimes Croats are given to feel that they are guilty of the atrocities of the Independent State of Croatia during World War II. Tudjman points out that this guilt should be balanced by the realization that Croats formed the majority of the early partisan movement and were therefore in the forefront of the resistance to fascism. He is probably correct in pointing out that the figure of seven hundred thousand deaths in the Jasenovac concentration camp is exaggerated, although why that should lessen the horror of the place is not clear. He also brings forward telling data on the concentration of Serbs in certain postwar agencies and in the Yugoslav economy.

But I do not want to give the impression that this is a balanced book. Tudjman is a nationalist, and his interpretations of Yugoslav history always favor Croatia. According to Tudjman, for example, the Bogomils were Croats; Bosnia and Hercegovina are Croatian lands; the "Croatian nation has aspired for centuries to realize its full national independence" (p. 120); since medieval times Croatia has never lost its nation-state identity (p. 121); the charges against Cardinal Stepinac are "inexplicable"; the coup d'état in Belgrade on March 27, 1941, sought to "insure the establishment of a centralist Yugoslavia after the war"; emigration and falling birth rates in the 1960s "threatened the biological existence of the Croatian nation" (p. 156); and so forth.

How would Tudjman solve the Croatian question? He does not really say, but one gets the impression that he would reinterpret the percentages agreement between Roosevelt and Churchill to correspond to the ancient Orthodox-Catholic division. The Orthodox portion of Yugoslavia would become an independent country dominated by

Serbs and allied with the Soviet Union, while the Catholic side would become an independent state dominated by Croats and allied with the West. Royalist Yugoslavia in 1918 and communist Yugoslavia in 1945 were purchased at enormous cost in human suffering. This does not necessarily legitimate those states, but it does suggest that the cost of putting into effect a solution that would be satisfactory to Tadjman might fray the edges of his humanism.

GALE STOKES
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STUART PIGGOTT, editor. *The Agrarian History of England and Wales*. Volume 1, part 1, *Prehistory*. New York: Cambridge University Press. 1981. Pp. xxi, 451. \$64.50.

This is the fourth volume to appear in a projected nine-volume history of agriculture in England and Wales from prehistoric times to 1939. The contributors persuasively demonstrate by their skillful interweaving of archaeological, botanical, and zoological evidence that, far from being a mere prelude or dim background to the historical period, the preliterate segment of British agriculture can justifiably be viewed as the first and vital stage of what was essentially a continuum of crops, livestock, techniques, and practices lasting into much more recent times.

The three contributors to volume 1, part 1, cover the period from the early fourth millennium B.C.—when agriculture was introduced from the Continent—to the eve of the Roman conquest in A.D. 42. (Volume 1, part 2, under a different editor carries on to the Norman Conquest.) In two initial chapters Stuart Piggott offers a reconstruction of the mesolithic environmental conditions that the neolithic colonizers encountered in the island and deals with the archaeological evidence for farming down to ca. 1900 B.C. In two long chapters constituting about half the volume, P. J. Fowler deals with "Later Prehistory" from the Beaker horizon down through the Bronze and Iron Ages to the second coming of the Romans. In a third substantial section of three chapters entitled "Livestock," M. L. Ryder, a zoologist, outlines what we know of the domesticated animals in prehistoric times, with some discussion as well of Scotland and Ireland and of British livestock in later periods.

Packed with masses of factual data, this book is not easy reading for nonspecialists, but there can be no doubt it is a treasure-house of information on the earliest British farmers. We have a picture of the development of prehistoric agriculture in Britain that must be the envy of scholars working in most other parts of the world. It is true that the overly

long gestation period of the volume (most of the manuscript was completed by 1975) and the unprecedented progress in archaeological research in Britain in the past decade now require that some statements and interpretations found here be modified or withdrawn, but the authors themselves point this out (pp. xix–xx, 278–79) and try to show what the innovations are and how they affect their conclusions.

Whether one is interested in specific facets of early British agriculture—types of food sources, technology and techniques, spatial distribution and architecture of the communities and their social organization, effects on the natural environment—or in the narrative and evolutionary side of the picture from the earliest but still mature economies based on pastoralism and cereals down to the competing chiefdoms based on intensive agriculture that the Romans found, this volume is a fine achievement. It should help dispel the notion of the pre-Roman British farmer as a primitive and inefficient one. This is also a well-made book with excellent illustrations, a useful list of radiocarbon dates, a good site index, and a bibliography that will be invaluable for other scholars.

PHILIP E. L. SMITH
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ROY E. SCHREIBER. *The Political Career of Sir Robert Naunton, 1589–1635*. (Royal Historical Society Studies in History, number 24.) London: The Society. 1981. Pp. viii, 190. \$38.50.

Nothing in Sir Robert Naunton's early career as a diplomat and civil servant foreshadowed the success he later achieved. (In an audience with the Danish king he became too tongue-tied to speak.) Only at the age of 54 did he finally come into his own as Secretary of State to James I. In his biography of Naunton, Roy E. Schreiber shows that Sir Robert displayed considerable ability as an intermediary between the king and Privy Council and in supervising members of the diplomatic service. He also became a spokesman of the anti-Spanish faction in the government.

Like other historians today, Schreiber rates James's political skill more highly than has been customary in the past. On the Council and in the diplomatic service he was careful to preserve a balance between advocates of a pro-Spanish policy and Puritan sympathizers such as Naunton who sought "the triumph of militant Protestantism both at home and abroad" (p. 125). By this means James kept the loyalty of moderate Puritans and parliamentarians who aspired to government office. At the same time he utilized members of both factions to serve his own ends. In 1621 he employed

Naunton to suggest marriage between Prince Charles and Henrietta Maria to the French ambassador. For Naunton this was a welcome opportunity to curb Habsburg power. For James it was merely a ruse to put pressure on the Spanish government to hasten negotiations for marriage between Charles and the Infanta. When the device failed, the French proposal was dropped and Naunton suspended from the Council at the behest of the Spanish ambassador.

Despite the disgrace he had suffered, Naunton remained loyal to the crown, and his reward was the lucrative office of Master of the Court of Wards. Schreiber gives a penetrating analysis of an institution that aroused great antagonism from the landed classes. Naunton lacked the forcefulness or legal training to be successful in this office, but under his rule the court at least checked the worst excesses of the wardship hunters.

When Charles I became king, Naunton had a brilliant opportunity, as an MP, to coordinate the common aims of king and Parliament in waging war against Spain, for he was known to have been "a martyr to the Spaniard's influence in England" (p. 115). He did not speak out effectively, however, and when conflict arose between Charles and the Commons, he adopted a neutral line between the government and the opposition with the result that he offended both and was expelled from the Council. Naunton's dismissal, according to Schreiber, illustrates Charles's abandonment of his father's wise policy of maintaining a broad-based administration. By alienating moderates like Naunton, the king divided the country and helped to bring on the Civil War. Though not always gracefully written, this is a thoughtful, well-researched study of a man whose chief characteristic was "quiet competence" (p. 53).

AMOS C. MILLER
University of Houston

RICHARD L. GREAVES. *Society and Religion in Elizabethan England*. Minneapolis: University of Minnesota Press. 1981. Pp. ix, 925. \$39.50.

The title of this formidable work is rather misleading, since the reader who expects a discussion of the close interplay of organized religion with Elizabethan society will be disappointed. There is little analysis, for example, of the social origins and status of the clergy, nor any attempt to discover the social basis of sectarian Puritanism or Catholic recusancy. There is practically no use of social science concepts, theories, or techniques. But the book is nonetheless quite valuable, and it represents an impressive scholarly achievement.

It is more intellectual than social history, in that

Richard L. Greaves primarily surveys religious opinion, Anglican and Puritan, on the subjects of marriage, sexuality, family, education, popular culture, economic relationships, death, and burial. This is popular intellectual history in its broadest (and probably necessary) sense—Greaves uses not only sermons and tracts but also spontaneously expressed opinions gleaned from court records and similar sources.

Here lies the principal methodological weakness of the book, in that the author admits in his preface that he had only very limited opportunity to use English manuscript collections, especially the often rich material found in diocesan and county archives. There is greater reliance on familiar printed sources. Yet the weakness is not insurmountable, since in the nature of his subject Greaves is attempting to establish broad patterns based on numerous particular instances. It is relatively arbitrary which particular instances are used, and it is not likely that wider sampling in local archives would significantly alter his picture.

There are no surprises here. Religious authorities had largely predictable opinions on most subjects, and differences among them were mainly a matter of emphasis. Like numerous writers before him, Greaves gets his feet stuck a bit in the quicksand of the Anglican-Puritan distinction. He finds several rather subtle bases for discriminating between the two, but like all such attempts it seems a bit circular—people are defined as Puritans because they held certain positions, and the positions are defined as Puritan because certain people held them. On the whole Greaves does not find major differences between Puritans and Anglicans on social questions.

The book might have benefited from being more speculative—attempting, for example, to assess the overall place of religion in Elizabethan society with respect to the long-term trends of secularization. But the modesty of its thesis is also its strength, since for a long time to come it will serve as the most thorough summary available of Elizabethan social beliefs as sanctioned by religion.

JAMES HITCHCOCK
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ALAN MACFARLANE. *The Justice and the Mare's Ale: Law and Disorder in Seventeenth-Century England*. In collaboration with SARAH HARRISON. New York: Cambridge University Press. 1981. Pp. xii, 238. \$19.95.

Alan Macfarlane continues his hounding of "the widely believed transition from peasant/feudal society to capitalist/modern during the three centuries from about 1450 to 1750" (p. 185). Having "removed" English peasants from the period and from the analytical clutches of Marx and Weber in *The*

Origins of English Individualism (1978), he looks now for further support in judicial evidence. The result is mixed but important, if only for the historical tale he tells.

William Smorthwait's "black and terrible troop" rode through the northern dales around Kirkby Lonsdale in the early 1680s, and they now provide Macfarlane with this case study. Documentation, however, remains almost entirely from the local JP who first tried the marauders and continued to pursue them, Sir Daniel Fleming of Rydal Hall, Westmorland. Fleming's papers arrived in the Cumbria Record Office, Kendal, in the 1960s: over 6,000 letters, plus memoirs, copy-books, a huge *vademecum* of personally annotated statutes, and, specifically, boxes 31–36 containing examinations, depositions, and warrants against the Smorthwait gang. All this fleshes out the skeletal record standard to judicial formulary, but only with the muscle constituting the prosecution's case. We see Justice Fleming vigorously acting as investigator and prosecutor in pretrial proceedings, which certainly vindicates John Langbein's arguments about JPs (*Prosecuting Crime* [1974]). We also get to read a string of modernized texts for virtually all depositions against the gang, often as a substitute for the author's narrative; but we still miss the case for the defendants, and this reveals much self-definition in Fleming's role as JP. Even more frustrating for legal historians, Fleming saved his bundles of pretrial evidence for the prosecution but recorded next to nothing about in-court trial proceedings.

The gang had been acquitted at the Quarter Sessions of 1683 before Justice Fleming, something Macfarlane barely mentions but that surely merits explication if judgment by contemporary peers mattered for anything. When an Assize jury at Carlisle in 1684 did convict William and Henry Smorthwait of burglary, it was before none other than "hanging" Judge Jeffreys. This greatly relieved Sir Daniel of fears for acquittal by yet another "favourable Jury of life and death," as his memoirs noted. And what, we may ask, does such a case study reveal about peasants, their existence or nonexistence in England? Here Macfarlane's arguments strain judicial evidence a bit further.

Classifying crime and criminals, peasants and otherwise, has become major sport for historians of late, and Macfarlane wishes "to go beyond E. P. Thompson's *Whigs and Hunters*" (p. 185). To do so, he divides offences into "peasant crimes" (homicide, rape, maiming, arson, animal-rustling) and "capitalist crimes" (theft, burglary, robbery, coinage violations), finding that the former "are very weakly represented" while the latter, specifically theft, "has been the central and most prosecuted criminal offence in England since at least the fourteenth century" (pp. 186–87). Such tidiness and quantita-

tive certitude rest entirely on his survey of northern Assize records for the century after 1650 and on a most peculiar "ratio of 'weaponless' inventories" (of what?) designed to prove that Kirkby Lonsdale's criminals were capitalists; that is, they did their deeds without weapons. If this is the sort of sense that the author makes of legal evidence, one must hesitate to suggest that he look further, in Chancery Lane and elsewhere, at plea rolls, equity bills, or even at a case-count for Kendal Quarter Sessions Papers.

The pity about this book, then, is that so fascinating a tale and so rich a record should be so uncritically presented. Its chief value is in its verbatim accounts from witnesses to criminal escapades and its definition of what one most aggressive JP could do in Restoration law enforcement.

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JOHN BOWLE. *John Evelyn and His World: A Biography*. Boston: Routledge and Kegan Paul. 1981. Pp. xvii, 277. \$32.50.

Considering the pre-eminence of John Evelyn in English history and literature as a diarist and virtuoso, it is surprising that he has received so little attention from biographers. The dearth can be explained in part by the lack of access to his personal papers, only made generally accessible to scholars since the Second World War. Moreover, it was not until 1955 that the full text of the diary was published, the previous versions containing only about sixty percent of the full text. John Bowle's life must be set aside W. G. Hiscock's *John Evelyn and His Family Circle* (1951) for a proper evaluation. One quickly finds that Hiscock's work is a much more informed and satisfying explication of the man. Indeed, Bowle's heavy reliance upon Hiscock's research in the Evelyn manuscripts can only be revealed by a comparison of the two texts. Hiscock's work is not mentioned in the introduction and cited only sparingly in the footnotes. Yet it clearly ranks almost equal to the diary itself as a basis for this newer biography.

Bowle's biography is at once a life and times, the times as seen through Evelyn's eyes by a series of short extracts from the diary with connecting narrative. The disconnected, summary entries in the diary are paralleled by Bowle's prose, which puts unrelated subjects together in the same sentence if not paragraph simply because they follow in that sequence in the diary. It makes for a very readable if disjointed introduction to the diary, but less so to an understanding of Evelyn. His personality, his diletantism, his excessive piety, his hypocrisy, his re-

served and retiring nature, and his long courtship of the sovereign and the great men at court for favors are not brought out in all that angularity and sharp relief that seem to best characterize the man. His relationship to his wife and his over-frequent absences from her are more strikingly delineated by Hiscock. Moreover, Hiscock's copious extracts from Mrs. Evelyn's letters reveal her to be very well educated, eloquent, as shrewd a commentator as her husband, and one less deferential or obsequious. It is a softer, less well-defined portrait that emerges here, closer to what Evelyn deliberately set out to establish in his diary than to what Hiscock's more searching study reveals.

Bowle's life is designed more for the general reader than the scholar, but given that qualification it is an entertaining, readable account of this priggish, rather unlovable gentleman. It is the public Evelyn, the writer and commentator, whom Bowle chronicles, a man who yet attracts our attention and interest because of his undoubted intelligence, his curiosity and range of interests, and his observation of Restoration England and the life of the court to which he was unwillingly, but irresistibly, drawn.

HENRY L. SNYDER
Louisiana State University

JOHN RULE. *The Experience of Labour in Eighteenth-Century English Industry*. New York: St. Martin's Press. 1981. Pp. 227. \$25.00.

The enthusiasm generated by E. P. Thompson's *The Making of the English Working Class* has stimulated new research, and a short book putting this scattered material together will be welcomed if it offers a balanced account and sets Thompsonian enthusiasms in the wider context. Thompson's own work not only suffered from inordinate length and undue concentration on the northern industrial regions, but it also was largely concerned with crafts that suffered decline in the first half of the nineteenth century. A longer perspective, taking in the golden age of some of the crafts in the earlier period, is much to be desired.

John Rule, a former student of Thompson's, exhibits commendable industry in assembling a mass of detail illustrating the exploitation of labor in all its many aspects—wages, hours, occupational hazards, insecurity, the end of paternalistic protection, and others—and in the variety of organized labor responses from crafts unions through restrictive practices to brutish violence and murder. The catalogue of misery allows no room for any consideration of artisan prosperity or opportunity. It is not that a more "optimistic" view is tried and found wanting; rather, such material is ignored as if it never existed. The mass of statistical material on

wages assembled by Flinn is ignored, while regions that prospered (like Birmingham) are barely mentioned or (like London) are considered from the angle of notoriously oppressed crafts (tailoring, dock work). Some of the industries selected for closer study may suffer from a selective view of evidence. For example, the commentary on frame-work knitting is based on a work published in 1935 and very largely superseded by more recent publications, while it is insisted that Devon declined after the 1720s despite clear statistical evidence to the contrary.

But what is ultimately so disappointing about this book is the narrow conception of the "experience" of the English working class. Surely that experience must be set in the context of the evolution of the workplace: the workshop, proto-factory, and fully evolved (Arkwright type) factory; the mining, furnace, and mill colonies; the growing variety, quality, and ingenuity of manufactured goods; the growing specialization of labor and rise of professional management. Surely it must emphasize the evolution of class structure: the gulf between the "labouring poor," the craftsman, and the assembly line worker; between the articulate craftsmen of the metropolis and the cheap deferential labor force of the Penines; between male and female labor. And surely any such study must assess the "civilising" influences at work (religion, education, improved housing) and the opportunities for social mobility in a dynamic economy.

Rule's study has much rich detail to offer, but it is a pity that he has not sought to extend his work from the exploitation to the experience of the English working class.

S. D. CHAPMAN
University of Nottingham

R. S. NEALE. *Bath 1680–1850: A Social History or A Valley of Pleasure, yet a Sink of Iniquity*. Boston: Routledge and Kegan Paul. 1981. Pp. xiv, 466. \$45.00.

In this attractive book R. S. Neale presents Bath as an artifact of the social order and culture of eighteenth-century England. Bath was built to satisfy the demands of the eighteenth-century elite, but its architectural, economic, and social structures fostered the emergence of a middling class that in the nineteenth century contested the elite's dominance. Because Neale's argument demands that he reveal the relations between Bath's development and the city's structures, the book is a fascinating delineation of an urban environment.

Obviously, a book on Bath must describe the English elite at play, and Neale's argument enables him to place their dalliance in a new context. In a

delightful chapter entitled "The Company and the Size of the Market," Neale relates the elite's expenditure in Bath to the city's growth and estimates that the elite's demands for enjoyment stimulated investment in domestic building roughly equal to the fixed investment in the eighteenth-century cotton textile industry. The funds for this investment flowed from a national and even an international financial market. So Bath represents the triumph of agrarian capitalism, and its first great architecture reflects the tensions of a developing market society. John Wood's squares at once celebrate the harmony of a pre-Lockean world and insulate the elite from those incapacitated from participation in civil society. Bath's beauty is that of an "urban utopia at odds with the world in which it was produced" (p. 209).

Neale's exploration of Bath's world involves examination of relatively unexplored areas of eighteenth-century urban history—of guilds and the regulation of tradesmen, of migration and administration of the poor laws, of criminal offences brought before the city's magistrates, and of the rise of residential segregation based on class. Mortality rates demonstrate that by the early nineteenth century Bath's lower orders were as wretched as those of Whitechapel, but Bath's tradesmen and artisans were an exceptionally prosperous group. And so the stage is set for the appearance of a middling class composed of nondeferential tradesmen and artisans. Neale has argued elsewhere that analysis of the middling class is essential to an understanding of radical politics in early nineteenth-century England. Here he examines the electoral base of a major philosophical radical, for in 1832 Bath sent John Arthur Roebuck to Parliament. In the 1830s Bath was a "radical utopia." But by the 1840s contradictions inherent in philosophical radicalism, contradictions stemming from the market society in which it arose, allowed Bath's upper orders to regain control of the city.

Is an argument that reaches its logical culmination in the story of the middling class an appropriate framework for a history of Bath? Neale's argument enables him to emphasize those aspects of Bath's history that symbolize changes in the nation's structure. But perhaps more analysis of phenomena that can be glimpsed only at the local level might have made mid-Victorian Bath's subsidence into "a welfareism which came close to being a substitute for political consciousness" (p. 320) less of an anticlimax. Neale's book raises interesting questions about the balance between national and local and between change and continuity in a local history. That is only one of the reasons why it always commands the reader's attention.

NORMA LANDAU
University of California,
Davis

R. S. NEALE. *Class in English History, 1680–1850*. Totowa, N.J.: Barnes and Noble. 1981. Pp. vi, 250. \$28.50.

This book, a collection of essays with more coherence than is common in the genre, can be read on two levels. Level one, the more concrete, embraces the central core of chapters. It involves a reassertion of R. S. Neale's five-class model of early industrial English society against what the author sees as three pervasive errors in the sociohistorical literature: first, a false judgment of eighteenth-century society as a one-class or no-class structure, instead of an emerging class structure not yet capable of full articulation of consciousness; second, the simple classification of early nineteenth-century society on a three-class basis; and third, the premature attribution of class consciousness to workers in this same period.

A subset of this first level involves two interesting chapters on expressions of class sentiment in the early nineteenth century, including expressions by women, which Neale persuasively interprets in terms of a society still in transition from orders, propertied and nonpropertied, to classes.

The book's second level involves a plea for more attention to theory—deriving from historical materialism but embracing also a systematic exploration of ideology—as against undue empiricism, unconnected topical excursions by social historians, and the theoretical mushiness of many British practitioners, among them some of the more renowned. To this end Neale explores Marx, Mannheim, a host of British surveys, and some less-known Japanese work by way of comparison. Neale is correct in reminding us of the necessity of social structural determination as both base and connecting tissue in social history, although some of his own excursions seem a bit long winded. Intending the book as a stimulus to question and debate, as a teacher's manual for younger scholars, he may also depend on too much prior knowledge. He may certainly be read as more hostile to empiricism than he perhaps intends. And he does not pay enough attention to the integration of valid research that is not specifically directed to problems of class; thus women are evoked with no reference to main-stream work in women's history.

The first, more precise level of the book is valuable stuff, though the chapters that involve the freshest material, on lesser-known social thought, are somewhat inconclusive as models for the rigorous use of ideas in social history. Neale is surely correct in insisting on the advantages of his five-class model. One only regrets that the presentation in this volume, in contrast to more detailed previous work by the author, is unduly abstract and unduly reified. It is curious to find an author referring to himself in

the third person. Nevertheless, if neither an introductory statement nor a clear advance on Neale's earlier interpretation, the approach does deserve emphasis, for as Neale suggests it has not been superseded by subsequent research.

PETER N. STEARNS
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JEAN-ALAIN LESOURD. *Sociologie du catholicisme anglais, 1767-1851*. (Publications Université Nancy II.) Nancy: Université de Nancy. 1981. Pp. 212. 113 fr.

It is a comparative rarity for a Frenchman to make a major contribution to the writing of British history, so it is especially pleasing to welcome this attempt by the late Jean-Alain Lesourd to apply the techniques of *sociologie religieuse* (so well developed in France by Le Bras, Boulard, and others) to the study of the English Catholic past. Utilizing a wide variety of primary sources—manuscript and printed, national and local, secular and ecclesiastical archives—Lesourd has provided us with a valuable sociological and quantitative portrait of the Catholic community in England during the last days of the penal laws (with particular reference to the 1767 papist returns, which are here analyzed in detail for the first time) and in the era of emancipation in the early nineteenth century.

Stimulating and erudite though the work frequently is, it is, unfortunately, not entirely without blemish. First, there is a notable lack of balance in the book's structure and content, an inevitable consequence of the fact that the volume, appearing posthumously, consists of a series of extracts, chosen by former colleagues at Nancy, from Lesourd's magisterial six-volume Strasbourg thesis on "Les catholiques dans la société anglaise, 1765-1865." The latter was presented in 1974 and has been available since 1978 from L'Atelier de Reproduction des Thèses, Université de Lille III.

Secondly, there are doubts concerning the accuracy and reliability of some of the statistics that, as in the case of estimates derived by the application of multipliers to figures of baptisms and marriage, are offered without adequate discussion of the methodological problems inherent in using them. With regard to the 1767 returns, it is puzzling to know why Joseph Massie's calculation of the social structure in 1760 was not considered as a basis for the classification of the occupational data. The account of the 1851 religious census is also a little disappointing and could have been improved by a closer acquaintance with the large body of secondary literature on this topic and by an examination of the original enumeration schedules in the Public Record Office.

Thirdly, Lesourd has been unduly modest in

avoiding any comparisons, whether denominational (with the experience of other English churches during this period), spatial (with other countries where Catholics constituted a persecuted minority), or temporal (with, for example, the research undertaken in Britain by the Newman Demographic Survey in the 1950s and 1960s).

Finally, there are many signs of carelessness in the work, not least in the footnotes; bibliographically incomplete citations, mixed French, English, and American spellings, misattributions, and misprints are all too numerous. Thus, for example, the name Faulkner appears as Falkmukes on page 199, while the reference in note 89 of page 197 is given as "????." Perhaps, for these and other reasons, most English-speaking readers are still likely to prefer John Bossy's *The English Catholic Community, 1570-1850* (1975) as the basic introduction to the subject.

CLIVE D. FIELD
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NORMAN CHESTER. *The English Administrative System, 1780-1870*. New York: Clarendon Press of Oxford University Press. 1981. Pp. 398. \$67.50.

This study of nineteenth-century British government is written by a retired practitioner and teacher of public administration. The book opens with a survey of the state of administration in 1780 (neither this date nor the terminal date of 1870 is adequately explained). Norman Chester particularly stresses that the system was highly diffused and rested on the concept of legal authority. Thereafter the process of change is explained by thematic chapters covering the crown, Parliament, officeholding, finance, departmental organization, and local government. The strongest parts of the book are those dealing with administrative and procedural developments that rendered ministers *politically* responsible to Parliament, with the transformation of "officers" into "employees," and with the internal organization of departments. It is far fuller on central than on local affairs, for which the author puzzlingly pleads lack of research. Chester is at his best narrating detailed administrative procedures and regulations, but once he moves into the field of political analysis his touch falters.

Presumably the potential audiences are those relatively uninformed persons requiring a guide to actual political and administrative practice. Yet few history teachers will be happy with the assertion that the 1832 Reform Act shifted power from the landowning classes to the middle classes, nor will they wish their students to think that it was based on the principle of one man one vote. Similarly, the research on the eighteenth-century Poor Law induced the hope that no serious scholar would reintroduce

the notion of Berkshire magistrates launching a new system in 1795, least of all one based on a fictitious piece of the legislation, the "Speenhamland Act." Well-established research findings also query the assertion that outdoor relief was more or less abolished by 1847. More important, the unsuspecting would never guess from Chester's frequently asserted principle of strong centralized control over the Poor Law that local autonomy and discretion, hence variability, were as much a feature of the post-1834 system as of its predecessor. These and other misapprehensions will cloud the issue; elsewhere the reader will be quite baffled. A horrendous misprint has the author claiming that populations rose to 15.9 million in 1841 "then declined and by 1871 had fallen to 5.4" (p. 70). Two pages later the reader is introduced to the teaser that, although taxation appeared higher in 1871 than 1780, it was really in population terms lower, for in 1780 taxation per capita was £1.2 while in 1871 it was "only £2.3 per head, still about twice the level of 1780" (p. 72).

So this is a book requiring a glossary of corrective guidance, but it will be useful as a data base, especially on central government administration in the mid-nineteenth century. What readers cannot fail to draw from this study is compelling evidence of the remarkable growth in size and function of the British governmental system. The question that has agitated the minds of historians since Dicey—why this expansion took place in a society wedded to political economy and individualism—does not seem to have bothered Chester. Implicitly his answer is that it was all a matter of bureaucracy.

DEREK FRASER
University of Bradford

MASSIMO DE LEONARDIS. *L'Inghilterra e la Questione Romana, 1859-1870*. (Scienze Storiche, number 28.) Milan: Vita e Pensiero. 1980. Pp. vii, 236. L. 17,000.

Massimo de Leonardis's study of England and the Roman Question will be of value to historians not only because it follows English policy toward Rome during a crucial decade of the Risorgimento, but also because, by virtue of its particular emphasis, it provides an interesting perspective on England's entire Italian policy. Scholars have advanced a variety of interpretations concerning the respective roles of balance-of-power concepts, liberal ideology, and religious considerations in shaping England's approach to the Italian national movement. De Leonardis recognizes and discusses the importance of all three, but he focuses especially on liberalism and Protestant hostility toward the papacy as determinants of English policy, despite the fact that at one point (p. 85) he ranks concern for international equilibrium ahead of ideological motivations in ex-

plaining why England chose to favor a unitary state after Garibaldi's successes in Sicily.

England's disdain for the papal government disposed it not only to accept unity, but, in 1860, also to hope for and favor it, because it would destroy the Temporal Power. Here is one of the keys, de Leonardis maintains, to understanding English conduct. He rides this thesis too hard at points but makes a telling case for his contention that the Roman Question was a central one for the English. He disputes Derek Beales's position that the action of the famous trio—Palmerston, Russell, and Gladstone—has been overrated. While pointing out those moments when the court and the rest of the cabinet prevented them from intervening too rashly, he argues that the three took advantage of cabinet divisions to determine the general direction of policy. Of the three, Palmerston had the most regard for considerations of equilibrium, Russell was more doctrinaire, and Gladstone was the most coherent in his ideas. The work examines the ideas not only of those three but also of many others, including Disraeli and Clarendon, and notes the differences in viewpoint between the Whigs and the Tories.

One of the major contributions of the book is its treatment of the ideas and actions of the Catholic minority, the relative neglect of which de Leonardis seeks to correct. Careful throughout to demonstrate the interrelationship of domestic and foreign policy, he shows diplomatic views being affected, especially in 1870, by the recognition that one out of five persons in the British Isles was Catholic. The influence of the minority was weakened, however, by its political divisions. Most members of its most effective pressure group, the Irish deputies, sought a solution of their country's problems through cooperation with the Whigs and voted for the Palmerston government. By bringing the Catholics onto the stage—Wiseman, Manning, Newman, and others—plus the Catholic press, de Leonardis gives the Catholic critique of English policy ample room for expression. He leaves no doubt about where his own sentiments lie: with the minority.

Although the drama of the story diminishes after 1860, the entire work is interesting, beautifully clear, and well organized. It is based on English and Italian archival and printed sources.

RAYMOND L. CUMMINGS
Villanova University

ANDREW T. SCULL. *Museums of Madness: The Social Organization of Insanity in Nineteenth-Century England*. New York: St. Martin's Press. 1979. Pp. 275. \$17.95.

Between 1750 and 1850, Andrew T. Scull argues, the English response to madness changed dramati-

cally. Before 1750 most mad people lived in the community; they were either maintained in work-houses or hospitals, or, more often, kept at home. By 1850, however, the deranged found themselves "incarcerated in a specialized, bureaucratically organized, state-supported asylum system which isolated them both physically and symbolically from the larger community" (p. 14).

Along the way a professional coup took place: doctors captured and organized the trade in lunacy. Although the prime movers in the early campaign to build a public-financed system of asylums for pauper lunatics tended to be laymen with either Evangelical or Benthamite sympathies, once these reformers began agitating for humane and rational care of the mad, doctors set out to win control of asylum management. The fledgling psychiatric profession insisted that madness was a somatic disorder and thrust itself forward as the group best trained to heal a diseased brain. Happily for the doctors, their subsequent failure to cure significant numbers of patients did no damage to their cause, for local authorities came to prize efficient custody over rehabilitation. Ultimately, Scull points out, well-run madhouses provided a handy dump for all sorts of troublesome people, thereby not only reducing the public's willingness to tolerate deviant behavior but also generating a broader conception of the nature of insanity.

Much less enlightening is the author's treatment of the forces that created a demand for asylums. Borrowing from the work of David Rothman, Scull contends that there was no causal link between urbanization and the growth of large asylums in England. Instead, he holds that the driving force behind a "segregative response to madness" was the advent of a mature capitalist market economy, one goal of which was to distinguish between the able-bodied (hence employable) and the physically or mentally handicapped pauper. Scull's case here rests on speculation rather than evidence. Why must employers have been "abstracting from the barely conscious activities of their fellow capitalists" (p. 72) to grasp the social utility of insane asylums? Englishmen intent on removing lunatics from society had only to note the expanding private madhouse system that Scull himself admits predated the rise of a national market economy.

Scull, a sociologist by training, makes plain his impatience with the "Whiggish" view of lunacy reform as a triumph of the humanitarian impulse. Quite properly skeptical of the reformers' rhetoric of intentions, he offers an alternative model that seems at best half convincing. Much is taken for granted in this book's preoccupation with model building. Was it mere accident that Quakers sparked the lunacy reform campaign? And can we assume, as does Scull, that the forces that gave rise

to England's public asylums necessarily shaped the social response to all forms of deviance? Theoretically bold, *Museums of Madness* would have profited from closer attention to historical fact.

GEORGE K. BEHLMER
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DONALD C. RICHTER. *Riotous Victorians*. Athens: Ohio University Press. 1981. Pp. xi, 185. Cloth \$14.95, paper \$5.95.

According to one observer, when Queen Victoria visited Birmingham in 1858 the crowds in the streets behaved as well as any aristocratic assembly in the queen's drawing room. Donald C. Richter's purpose is to provide a salutary reminder that, even during the 1860s, 70s, and 80s, "the public orderliness of Victorian society is a great misconception" (p. 163). His evidence includes the Hyde Park riots of 1866 and 1867; the disorders sparked by William Murphy, the anti-Popery lecturer, in the 1860s and early 1870s; the disturbances ignited by Salvation Army processions in the late 1870s and early 1880s; and some "seventy-one separate incidents of serious disorder which broke out in conjunction with election campaigns from 1865 to 1885, each causing substantial property damage and/or personal injury" (p. 67).

The best and longest chapters, "The Pall Mall Affair" and "The Struggle for Trafalgar Square," recount the manner in which socialist attempts in 1886 and 1887 to arouse the unemployed were transformed into a struggle with the police over the right to use the square for giant public demonstrations. In these chapters Richter vividly yet judiciously conveys the interplay of the attitudes and the actions of demonstrators, administrators, and spectators alike.

Several of the earlier chapters are surprisingly thin, however, and replete with undocumented introductory generalizations. How much evidence is there, for example, for the statement that in "western England, Wales, and Scotland, the Irish and the Orangemen fought the Battle of the Boyne again and again through the centuries" (p. 20)? In other cases, relevant secondary works are ignored. It may be immodest of me, but I find it distinctly odd that my article in the September 1975 issue of *Victorian Studies*, "The Murphy Riots: A Victorian Dilemma," goes unmentioned, even though it is twice as long as is Richter's chapter on the same theme. He employs many of the same (doubtless independently discovered) quotations, and he reaches analogous conclusions—and he served as article referee.

Richter's concern is less with the rioters than with the forces of riot control: the metropolitan and rural police, the militia and the regular army, and the

home secretaries who sanctioned military aid when a local disorder got out of hand and who time and again sought to balance the ideals of public order and freedom of assembly. Richter has familiarized himself fully with their surviving personal papers (as well as with the comprehensive Home Office files and much else). On balance, he gives them high marks for fairness. At a time when other historians have emphasized the oppressive features of Victorian "social control," Richter finds the Victorian authorities remarkably permissive. Victorians en masse were neither murderous nor revolutionary, but, rather more often than we may be inclined to recall, they enjoyed a good fight.

WALTER L. ARNSTEIN
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J. A. MANGAN. *Athleticism in the Victorian and Edwardian Public School: The Emergence and Consolidation of an Educational Ideology*. New York: Cambridge University Press. 1981. Pp. xv, 345. \$49.50.

Institutional change is never without paradox. The celebrated manliness and sense of corporate loyalty caught in the tag "play up and play the game" of the English public school developed in the mid-Victorian period when presumably individualism was valued. Conversely, respect for the autonomy of the school boy developed after the First World War—very noticeably at new schools like Stowe—when political and economic laissez-faire was being scrapped. J. A. Mangan, however, is not really concerned with paradoxes such as these. He does not move much outside the schools to consider the growth of games generally as a feature of Victorian consumerism (as noted by another reviewer) or to the extraordinary preoccupation with health, physical fitness, death, and decay, recently discussed by Bruce Haley in *The Healthy Body and Victorian Culture*, which helps explain the reforming work of such headmasters as Almond at Loretto. But as an internal account of the rage of athleticism, of the forms that it took with accompanying rituals and symbols, of the system of school values that took two generations to create and longer to dissipate, his study is serious and illuminating. It holds the reader's interest exceedingly well.

Six representative schools from six distinct classes of institutions are examined: Harrow as a traditional great public school; Stonyhurst, the Jesuit foundation, as a denominational one; Marlborough as a proprietary school; Uppingham as a revitalized grammar school; Loretto as a private venture school; and Lancing, with its magnificent Victorian chapel, as one of the Church of England schools

founded by Canon Woodard. Mangan finds similarities of structure and problems that led to an emphasis on collective physical activity, but he is careful to note and elaborate upon differences of purpose and circumstance. The historical outcome may be similar in each of the six, but the route traveled was not always the same.

Among Mangan's more important conclusions, which challenge and correct opinions expressed in what has long been a formidable literature, are these: (1) The reason for adopting games varied from institution to institution. At Loretto health was a factor, at Stonyhurst stamina, elsewhere athletics was a form of social regulation either to curb schoolboy license or promote school identity. (2) Headmasters initially controlled the tempo of change. Mangan disputes the popular contention that games owed most to the initiative of the boys themselves. Games were therefore a form of institution building. (3) The house system of residence underwrote the success of games. In time headmasters found themselves helpless before housemasters. (4) The dominance of games was not a spontaneous achievement. Boys and even some masters resisted, especially at the outset. A staggering amount of coercion, intimidation, and humiliation maintained the system. Selwyn at Uppingham, for example, used his gold-headed stick to force boys to shout at matches. This was only possible within what sociologists call a "total institution." (5) Contrary to the usual view, new schools did not take the lead in promoting games and athletics. Pride of place may have to go to Harrow.

Mangan's final words are judicious. The public school rhetoric of morality may have been "naive, narrow and muscular," but it was not ignoble. "It may now be time to acknowledge its virtues rather than to continue to mock its shortcomings." No doubt, but the philosophy of education through pain that the rituals of games and athletics required, as in some primitive initiation rite, will not find sympathy in cultures based on wholly different premises.

SHELDON ROTHBLATT
University of California,
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JILL FRANKLIN. *The Gentlemen's Country House and Its Plan, 1835-1914*. Boston: Routledge and Kegan Paul. 1981. Pp. xvi, 279. \$40.00.

First, there is the inevitable question: how does this book complement the well-known recent works on the subject by Mark Girouard? Jill Franklin concentrates on one essential aspect: the plan. Of the 1,000 to 2,000 country houses built in England and Wales

during the Victorian and Edwardian periods she selects 380, out of which 70 plans are shown—unfortunately not on the same scale, so that a comparison of the sizes is made rather difficult.

As far as possible, the author indicates the functions of all the rooms in these houses. Her subject is really the use of the country house in the nineteenth century: what kinds of rooms were needed (for instance, the rise and decline of the smoking room), how they were placed in relation to the whole, what was the precise role of the members of the family and the servants. The evidence is plentiful; apart from the plans themselves there are the contemporary building handbooks, the building press, and foreign observers. But many of the most interesting facets lie hidden in little-known memoirs or in the asides of voluminous Victorian novels. In carefully piecing together this information, Franklin has made a very valuable contribution not only to architectural history, but also to the social history of the Victorian age.

Probably never before, and certainly never after that period, could a house for one family be so complicated, could there be the same degree of differentiation of the use of rooms and so much segregation among the different groups of inhabitants. There were, in fact, two major, conflicting tendencies. Life and comforts became more elaborate; thus more servants were needed, and could be had. On the other hand, there was a growing desire for privacy; hence the increasingly strict segregation of masters and servants. At Peckforton, in the 1840s, the kitchen was placed at a distance of 180 feet from the dining room. The hierarchy of propriety among the servants themselves was equally strong, which could lead to astonishing situations where, for instance, the master of the house had to light the fire for a guest because the servant to whom this task was assigned was absent, and no other servant would touch that job (p. 87). Later on, some large houses added a "private wing" or "family wing," so as to be away from the bustle of reception and the like. By the early 1900s, many owners rejected size and ostentatiousness *per se*.

Basically, Franklin claims, the functions of the servants' quarters remained the same from the late eighteenth century onwards. Because of the angle she chose, particular architectural styles of decoration matter less in her story of the country house. It was probably in this area, in the broad development from Classical to Gothic and then to the revivals of vernacular styles, that the greatest changes occurred; there is room for another detailed book on the subject.

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J. D. MARSHALL and JOHN K. WALTON. *The Lake Counties from 1830 to the Mid-Twentieth Century: A Study in Regional Change*. Manchester: Manchester University Press. 1981. Pp. xii, 308. £13.50.

This original study of the Lake Counties deals with the modern county of Cumbria (formerly Cumberland, Westmorland, and the Furness district of Lancashire). The authors argue that this region possessed, and possesses, distinctive economic, political, and social attitudes and organizations. All but the final chapter explore themes in the period from 1830 to 1914. J. D. Marshall, who writes the first seven chapters, led the Centre for North Western Studies at Lancaster University, and he draws upon his own formidable knowledge as well as that of scholars associated with the Centre. John K. Walton takes responsibility for the rest of the book, adding to our understanding of leisure, his special field.

The first chapter sets the scene during the twenty years after 1830: railways arrived, products of the region increased in variety, and many found markets not only in the British Isles but also overseas. In the second chapter, the changes in the economic structure between 1830 and 1914 are shown to correspond to the availability of external investment and to wider economic influences, so that some traditional industries, such as textiles, declined while heavy engineering flourished. Agriculture, in the third chapter, receives special consideration because it remained throughout the period the largest employer; its prosperity and those responsible for its fortunes receive sympathetic discussion. Social attitudes and relationships form the subject matter of the next five chapters: migration, social classes and property, education and social institutions, culture, and the tourist industry. Although many of these themes have appeared in recent histories of Britain, the focus on a particular region heightens our awareness of how changes occurred and their implications. Thus, for example, political movements in the nineteenth century had peculiar local features that show the counterpoint of influences from within Cumbria and without that allowed socialist organizations to emerge. The ninth chapter, on the defense of Lakeland, examines the period until about 1914; it illustrates the conflicts between those seeking to preserve or to conserve beautiful scenery and a traditional lifestyle and those concerned with mineral wealth, water for the conurbations of Liverpool and Manchester, and tourism.

The epilogue sketches the decline since 1914 of many parts of the economy that had prospered in the nineteenth century. Conflicts still persist among the interests of tourism, modern industries such as nuclear energy, and the protection of the environment.

This book ought to be read by anyone studying nineteenth- or twentieth-century British history because it takes people as the proper subject for examination on a scale that enables us to glimpse the horizons that bounded many of them at work and at play. It is a pity that the book lacks a select bibliography, although a patient use of the footnotes will allow readers to take inquiries further. Some of the captions could have provided a little more helpful information than "a detached shippon and the double-pile farmhouse" for picture number 9 (p. 64).

IAN KEIL
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R. MERFYN JONES. *The North Wales Quarrymen, 1874–1922*. (Studies in Welsh History, number 4.) Cardiff: University of Wales Press. 1981. Pp. x, 359. £14.95.

The only time the slate quarriers of North Wales secured the attention of the British labor movement was between November 1900 and December 1903 when a protracted lockout at the Penrhyn quarries was fought to its bitter, destructive end. Although it was not recognized at the time, this event signaled the beginning of the end for the industry and its workers: demand for slate plummeted and the markets supplied by the largest quarry in the world collapsed. But in this finely detailed and sensitive study, R. Merfyn Jones goes far beyond a narrative account of frequently turbulent industrial relations to provide a wide-ranging description of the religious, political, and workplace history of the slate-working communities.

For one hundred years, these relatively isolated outposts of "internal colonialism" had provided Britain with all of its roofing slate and had defiantly preserved their Welsh cultural identity in the face of the paternal tutelage of Anglican and English entrepreneurs. Just as members of the English aristocracy of labor prided themselves upon their independence, so the slate workers (whose skill derived from an experiential knowledge of the vagaries of the rock) rigorously defended their independent quarrier status. From the late 1870s, however, the bastions of this independence came under increasing pressure as employers schooled by London-based cost accountants responded to falling profit margins by undermining those traditional methods of work organization that had secured independence for the men and imposing a modern industrial discipline through closer regulation of work and leisure. The bargain system of wage negotiation was slowly replaced by subcontracting, the property rights of the men to certain parts of the rock were attacked, and a tighter control over time was enforced by efforts to

end customary holidays and irregular work habits. Out of the opening shots in this campaign, at Penrhyn in 1874, the union was born. But as Jones shows, the union was a mere shell of an organization—run for twenty-five years by a middle-class chartered accountant out of his office—and the real basis of worker strength and action lay in the informal *cabans*, work group solidarities of the quarry gangs, reinforced by such cultural institutions as the chapels and the impenetrable wall of the Welsh language. Not until the denouement of 1903 did the union revert to working-class leadership, develop a more centralized governmental structure, and begin to play a more active role in industrial relations.

Significantly, by that time the cultural defences that had sustained the men in their battle for independence had crumbled. For the industrial struggle was more than a continuing conflict between rationalizing capitalists and preindustrial workers. Beneath the combat and mediating the class relations lay the clash of two cultures. Once the industrial battle was lost, the community also suffered a decisive defeat. About 1,000 men out of 2,500 left Penrhyn; many of those who returned to work were not the original workers; chapel attendance fell; and, most revealing of all, the 1904–05 religious revival left Penrhyn relatively untouched. On the other hand, laborist socialism was firmly implanted by the experiences of these years.

Jones does a fine job of integrating trade union history with the structure of the workplace and the wider cultural context. He deftly sketches the "pre-industrial" context of community solidarity and reveals it not as some residual formation but as central to the quarry workers' social relations of production. This book is an impressive addition to the new scholarship on Wales; but more than that, it is a practical example of how to carry through Hobsbawm's injunction to write the history of labor in society.

RICHARD PRICE
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THOMAS C. KENNEDY. *The Hound of Conscience: A History of the No-Conscription Fellowship, 1914–1919*. Fayetteville: University of Arkansas Press. 1981. Pp. ix, 322. \$20.00.

Among the dozen or so antiwar groups in Britain during the First World War, the No-Conscription Fellowship (NCF) was the sole organization to attempt to obstruct the war effort. Formed originally to oppose the adoption of a conscription bill, the NCF, after passage of the Military Service Act in 1916, shifted its efforts to providing advice, support, and organization for those who refused to serve in

the army on grounds of conscience. Using materials from a wide range of private collections and official documents, Thomas C. Kennedy has produced a balanced and useful study of this group of highly principled dissenters.

After brief but helpful sections dealing with the history of enforced military service in Britain and the political maneuvering that led to the adoption of conscription, Kennedy focuses on the organization, activities, and policy debates of the NCF. Financed by wealthy Quakers, the organization was nonetheless a haven for nonreligious objectors whose refusal to perform military service was based on moral or political grounds. The heterogeneous nature of the membership led to continual disagreements. Although the leadership recommended refusal to perform even nonmilitary alternative service, a course that led to imprisonment, the majority of the rank and file ignored this advice and agreed to do work of "national importance." The Quakers, who saw the conscientious objectors as silent witnesses, resented the NCF's efforts to improve their treatment at the hands of the government, while the socialists, impressed with the social results of the Russian Revolution, began to question the organization's absolute renunciation of the use of force.

While the broad outlines of this story have been known to historians familiar with the dissenting groups of World War I, this work provides the full story of the anticonscription movement for the first time. Kennedy's treatment both of the NCF and of those charged with the enforcement of conscription is scrupulously even-handed. Although he gives full credit to the anticonscriptionists for their undoubted sincerity, courage, and hard work, he concurs with one of their leaders, Clifford Allen, in faulting their "spiritual pride" (p. 279) and the continual quarreling over narrow issues that limited their effectiveness. The members of local tribunals, often pictured in memoirs of pacifists as abusive jingoes, were, Kennedy argues, more often the victims of inadequate or confusing directives, and he demonstrates that diligent efforts were made by military authorities to prevent the mistreatment of those conscientious objectors, including most of the original NCF leadership, who chose prison. This official sensitivity was in part the result of the efforts of such upper-class leaders of the NCF as Catherine Marshall and Bertrand Russell, who used their contacts to establish a curiously close working relationship with the government whose war effort they were attempting to obstruct.

In their objective of bringing the war to an end by making conscription unworkable, the NCF clearly failed. Kennedy wisely resists any impulse to argue any long-term significance despite this defeat. While the defense of the principle of freedom of conscience by the anticonscriptionists was edifying,

these quarrelsome pacifists were, he concludes, simply ineffective.

CATHERINE ANN CLINE
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DONALD S. BIRN. *The League of Nations Union, 1918–1945*. New York: Clarendon Press of Oxford University Press. 1981. Pp. 269. \$55.00.

The League of Nations Union (LNU) was the product of middle-class liberal internationalism that found in the idea of collective security the terms to renew its progressive faith. As the institutional focus of popular hopes, the LNU won a million members by 1930. This book is a detailed account of the organization and its leadership.

The problem with the LNU was that, despite the enthusiasm of its members, it was tied to a corpse. The rhetoric of internationalism never provided a viable program. Popular hopes, bewitched by the idea of collective security, hardly acknowledged the intractable limits imposed by the continued existence of autonomous states with which governments had to deal. Without functional policies and without a party base in Britain, the LNU had little political influence. Donald S. Birn rightly criticizes Robert Cecil and Gilbert Murray for not giving their followers a realistic evaluation of the League. They, and the LNU membership, continued too long to attribute to the League qualities and capacities it did not have. Their admiration of the mandate system, for instance, expressed an ideal of enlightened colonialism rather than an accurate picture of the weak mandate commission. Not a single commissioner ventured forth from Geneva to see the actual situation of the mandated territories. In like vein, neither Cecil nor Murray correctly assessed the political world beyond the British Isles. The gap between LNU prescriptions and world reality led, naturally, to confusion and frustration.

Birn shows the limited, sectarian base of the organization, its insular understanding, and its remoteness from events. Although he does not stress the point, the LNU can sensibly be interpreted as an episode in modern British politics in which a disarrayed liberal center sought new purposefulness in Wilsonian idealism. The LNU was its lobby, a substitute party centered on a high-minded quest for world order. An implication in Birn's conclusions, that it was the League that let the LNU down, misses the point. The League was flawed from the start. Insofar as this was not seen or accepted by the leadership of the LNU, it condemned the institution to ideological frustration and practical impotence. This Birn shows clearly.

The sustaining myth of collective security fell in the 1930s. Birn's most interesting chapters show

how the leadership recognized that "League opinion" was not enough, that collective security had to be redefined, and that there was a need for rearmament. But they had no monopoly on this idea, and they were determined at the same time to hold on to the old faith of the Covenant. This perplexed the membership. Support ebbed. The story of the 1930s for the League and the LNU is one of incoherence and decline.

This is as complete a study as we will need of the inner workings of the organization, the nature of its support, its political failure, its educational program, and its leadership. It is hard to imagine the LNU without Cecil.

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DAVID CARLTON. *Anthony Eden: A Biography*. London: Allen Lane; distributed by Indiana University Press, Bloomington. 1981. Pp. 528. \$35.00.

Honestly self-described as an "interim" biography that will need amendment as additional records and private papers become accessible to researchers, David Carlton's strikingly formulated evaluation of Anthony Eden's public career presents an unmistakably revisionist perspective on that modern British statesman whose special destiny it was to have had major responsibility for his country's foreign policy during three climactic episodes along a trajectory of decline. In addition to showing in a general way that Eden was a "selective" practitioner of appeasement who regularly overestimated British power and who therefore counseled some reckless actions over the years, the revisionism of this treatment emerges most prominently in the analysis of Eden's relationships with Neville Chamberlain, Winston Churchill, and John Foster Dulles.

With respect to Chamberlain, the most salient argument is that Eden—by virtue of his somewhat specious identification with the League of Nations and with "moderate" or "centrist" opinion—posed a sufficiently powerful political counter-attraction to the prime minister after Prague to cause the Cabinet, led by Halifax, to embark on the territorial guarantees and the negotiations with the Soviet Union that eventuated in both Great Britain's accelerated descent from great power status and the bolshevization of Eastern Europe. In the case of Churchill, the point is to show that those accounts that have emphasized harmony and reciprocal affection between the two men distort the true picture, which is one of protracted rivalry punctuated by frequent disagreements over policy and, on Churchill's part, by some acts of almost gratuitous cruelty, especially as regards the long-delayed suc-

cession. As for Dulles, Carlton here makes a highly persuasive case for his innocence of the heaviest charges in Eden's published indictment; only someone as willful and politically desperate as the prime minister had become by mid-1956 could have misunderstood so grievously the American position in the run-up to the Suez Crisis. Add to this evidence of Eden's naive confidence in the compatibility of Soviet and Western purposes long after Churchill had concluded otherwise, and the grounds for a substantial revision of what may still be conventional views are compelling in their scope and documentation.

Although these summative judgments limn a portrait of Eden as public man that no subsequent historian will want to ignore, the cool, austere perspective that controls the work invites comment on two points. Firstly, it may be wondered whether a study that manifests practically no sustained interest in Eden's personality and that makes no consistent effort to analyze his policies in detailed relation to alternatives that may have existed at the time will carry conviction with critical readers. Secondly, it may be asked whether it is ever appropriate, except possibly for heuristic reasons, to impute to historical personages responsibilities for outcomes that, when crucial matters awaited decision, were still densely veiled in the mists of time. However stimulating and cogent Carlton's interpretation may be—and it clearly is both—its performance on the points just mentioned makes it highly likely that it will figure in the literature on Eden much as Robert Rhodes James's study of a decade ago has come to figure in the literature on Churchill.

DONALD LAMMERS
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DAVID STEVENSON. *Scottish Covenanters and Irish Confederates: Scottish-Irish Relations in the Mid-Seventeenth Century*. (U. H. F. Historical Series, number 5.) Belfast: Ulster Historical Foundation. 1981. Pp. xii, 364. £15.00.

This is an important book, not merely in regard to its relatively narrow focus—the Scottish army in Ulster during the 1640s—but also in regard to the broader question of the interconnection of the three kingdoms of the British Isles. David Stevenson, the author of two recent (but already standard) books on the Covenanters' rebellion in Scotland, is at home with Irish and English sources as well as Scottish, and *Scottish Covenanters and Irish Confederates* is the most significant study of Anglo-Irish-Scottish relations during the middle decades of the seventeenth century.

The book opens with a discussion of the background: the connection between the Scots in Ulster

and Scotland had long been primarily an extension of clan feuds, especially between the MacDonalds and the Campbells. Throughout these years of high politics, of religious disputes and constitutional crises, the MacDonalds' desire to regain Scottish lands often influenced the course of events. The central part of the book is a detailed study and evaluation of the organization and significance of Major General Robert Monro's Scottish army, which landed in Ulster in 1642. The Confederate Catholic rebellion had begun the previous year, and Monro's forces were commissioned by Charles I, paid (off and on) by the English Parliament, and apparently received orders from Scotland. Stevenson picks his way surely through a maze of conflicting motives, as Parliament, the king, and the Scottish government at various times hoped to use the Scottish army in Ulster to influence events in England and Scotland. Matters became even more confused in 1647 when a segment of the Scottish leadership, known as the Engagers, signed a treaty with Charles that, among other clauses, promised to pay Monro's army its arrears. A portion of the army, therefore, returned to Scotland before Cromwell routed the Engagers at Preston. Charles I's execution in January 1649 produced some extraordinary alliances. Royalists and Scottish Presbyterians in Ireland united briefly against Parliament represented by Colonel George Monck, who responded by making the most unexpected alliance of all, with Owen Roe O'Neill and the extreme Irish Catholics. Stevenson controls his material with such expertise that each new strand in the web of religious, political, and personal motivations becomes clear to the reader.

Earlier historians considered the Scottish army in Ireland as insignificant militarily and rapacious in its relationship with the civilian population. Stevenson, however, argues that "the army did have considerable achievements to its credit . . ." (p. 298). In light of the facts that Monro lost his only major battle and that contemporaries were almost unanimous in criticizing his forces, Stevenson's conclusion may be too favorable. But as Stevenson points out, the very presence of the army may have preserved the Ulster Plantation. "Under its patronage," Stevenson argues, "presbyterianism became so firmly rooted in Ulster that it proved impossible to eradicate it" (p. 303). Irish history is full of ironies, but the greatest may be that an army that had little military success helped create a pattern that still influences Irish politics.

DOUGLAS G. GREENE
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JAMES D. YOUNG. *The Rousing of the Scottish Working Class*. London: Croom Helm or McGill-Queen's University Press, Montreal. 1979. Pp. 242. \$23.95.

WILLIAM M. WALKER. *Juteopolis: Dundee and Its Textile Workers, 1885-1923*. Edinburgh: Scottish Academic Press; distributed by Columbia University Press, New York. 1979. Pp. iii, 561. \$20.00.

These two books are very different. *Juteopolis* consists of the detailed study of one neglected working-class group, the textile workers of Dundee. Having identified the chief characteristics of the city during the late nineteenth century (including its geographical isolation, dependence on textiles, and high proportion of women workers), William M. Walker devotes the main body of the book to a scrupulous examination of the textile workers' struggle for effective trade unionism and the impact that this had on the industrial and political life of the city. He explores the career of the paternalist Reverend Henry Williamson and his Mill and Factory Operatives' Union, an organization whose weaknesses were exposed so ruthlessly by the strike of 1895. The author then proceeds to an examination of the militant and class-conscious Dundee Jute and Flax Workers' Union that was founded in 1906. (The differences between the two unions, Walker points out, were not dissimilar to those between New Model and New Unionism.) Under John Sime's professional and self-assured leadership, the Jute and Flax Workers' Union expanded its membership, organized its own insurance scheme, secured recognition from the employers, and became increasingly involved in political activity. This brought the union into close contact with the Scottish Prohibition party and its leader, Edwin Scrymgeour, whose extraordinary career culminated in the defeat of Winston Churchill in the general election of 1922.

Although the author modestly disclaims any intention to build theoretical models or to undertake a comparative study, he does much more than simply provide new information about the Dundee working class: he has some sharp comments to make about alienation, respectability, false consciousness, and the heroic school of labor historiography. Making excellent use of the local press together with the records of both unions and employers, Walker's well-written, though perhaps overlong, book demonstrates the value and vitality of what may appear at first sight to be a rather old-fashioned format: the detailed, narrative, largely institutional history of a single group of workers in a single city.

James D. Young's aim is altogether more ambitious—"to chronicle the story of the Scottish working class between 1770 and 1931" (p. 10), and this in fewer than half the pages that it took Walker to deal with less than forty years of working-class history in one city. The central concern of the book is most important and revolves around the apparent contradiction between a docile and moderate radical

movement and an unruly and unstable working class. Thus the title is somewhat misleading, since the book is about the *failure* of the labor movement to rouse the working class. This failure is to be explained, according to Young, by the way in which a provincial bourgeois elite destroyed Scottish working-class culture. Drawing on Michael Hechter's concept of internal colonialism ("the political incorporation" of a culturally distinct group by 'the core' " [p. 72]), Young argues that the combination of English capitalism and the Scottish Enlightenment had a devastating effect on the making of the working class, leading to linguistic insecurity, sexual repression, and a belief in provincial inferiority and backwardness.

For all its ambition—in fact precisely because of it—this book is markedly less successful than *Juteopolis*. Young's analysis, though thought-provoking and initially attractive, is neither sustained nor based on solid empirical foundations. Far more evidence needs to be produced before it can be accepted that, for example, there was a "revolutionary situation" in 1820 (p. 62), that by the early 1830s "the Scottish working class was already the most militant, class conscious and politically aware working class in Europe" (p. 81), or that during the First World War "a sense of national identity was forged in the heat of class struggle by native Scots, Irish immigrants and Highlanders" (p. 191). Nonetheless, by their contrasting yet complementary approaches, these two books do help to clarify the complex making of the Scottish working class.

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DONALD R. KELLEY. *The Beginning of Ideology: Consciousness and Society in the French Reformation*. New York: Cambridge University Press. 1981. Pp. xv, 351. \$49.50.

This wide-ranging and thought-provoking book builds upon Donald R. Kelley's own earlier and more tightly focused studies of aspects of sixteenth-century French legal, political, and historiographic history. It reports on much fresh research, particularly in sixteenth-century French pamphlets. And it supplies some intriguing and rather personal reflections on the development and enduring significance of the Reformation in France, reflections that command respect because of the author's knowledge and sensitivity.

Kelley defines ideology as a "more or less coherent conglomerate of assumptions, attitudes, sentiments, values, ideals and goals accepted and perhaps acted upon by a more or less organized group

of persons" (p. 4). Ideology is thus for him both more and less than a philosophical or theological system. It must be studied, furthermore, within a firmly established social context. The ideology upon which Kelley concentrates was developed by Protestants in France between 1534 and 1573. The context within which it grew he breaks down for analysis into separate chapters on the family, the religious congregation, the academy, the legal profession, the new medium provided by printing, and the politico-military party. These topical chapters are framed, at the beginning and end, by more general chapters on the origins and decline of this particular ideology. Each chapter begins with an epilogue that describes artfully one dramatic incident that is particularly revealing of the chapter subject. Each epilogue is then followed by a logically organized set of rather general reflections, occasionally extending beyond France and even beyond the sixteenth century.

The freshest factual information this book provides is drawn from close examination of pamphlets, some relatively neglected, others already widely studied—notably the statements of radical political thought provoked by the St. Bartholomew's Day massacres. Kelley goes beyond pamphlets for his raw material, however, drawing on correspondence, chronicles, martyrologies, and other sources to give depth to his picture. The freshest conceptualization this book provides is drawn from the social sciences, particularly anthropology, but also sociology, political science, and psychology.

Any book of this range must be a bit uneven. Kelley seems to me most sure and authoritative when he is dealing with legal and political and historical thought, a shade less certain when he is dealing with theology and printers. But his general standard is very high indeed. No one interested in the period of the Reformation should ignore this book; many interested in other periods would find it profitable.

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T. J. A. LE GOFF. *Vannes and Its Region: A Study of Town and Country in Eighteenth-Century France*. New York: Clarendon Press of Oxford University Press. 1981. Pp. vii, 445. \$74.00.

Even in the midst of the variety and localism of the Old Regime, Brittany stands out from the rest of France. Its Celtic language, systems of land tenure, and devotion to clergy and counterrevolution made Brittany unique. T. J. A. Le Goff has analyzed one small part of that perplexing province in this book. He has chosen—wisely, I believe—to deal only with certain limited questions about the Vannes region

rather than attempt to explain all aspects of the place during a century.

The first third of this book is a discussion of the town of Vannes and the remainder of the *pays vannetais*. The discussion of the countryside is superb, reflecting solid research and keen understanding of the documents. Not only has Le Goff chosen important questions to pose, he writes well in answering them.

One of the most important areas of study for Brittany must be the church, because of the traditional image of the province as a region devoted to church and clergy. In light of the detail presented on the geographical and social origins of the clergy and their relations with the Bretons, this image seems to be a valid one. There are very few surprises given us about the church aside from the high percentage of clergy recruited from the countryside, but even this is not atypical for the more rural provinces.

One of the most exciting parts of Le Goff's study is the chapter entitled "Rural Society from Within: Insiders and Outsiders," based on imaginative use of such sources as depositions in criminal investigations. The result is a fine discussion of identity, family and marriage patterns, and ordinary social intercourse. A clear image emerges of a society that was prone to violence (or at least threats of violence), drunkenness, and collective amusements such as dancing and ball playing. It was a society that valued family highly but, it seems once again, did not have extended families.

Perhaps the most important part of the book is the analysis of the *domaine congéable*, a system of land tenure in which the nobility owned almost all land. However, the tenant owned all edifices and could be removed from the land only with considerable difficulty. There were many peasant families who had farmed and lived on the same land for generations, paying a nominal rent to the noble owner. Technically these peasants owned no land. In actuality they were often well to do and had the mentality of proprietors. This system of land tenure, perhaps even more than the ubiquitous church, turned the *Vannetais* against the Revolution, for the laws of the National Assembly favored the land owners.

My only real complaint about the second part of this book is that the charts do not add much to the text and at times border on the trivial. Others, however, may find that information of more interest.

The first part of the book is less worthwhile. It purports to be a presentation of the urban center of Vannes. In fact, it says little of regard. The urban social system is not made clear. The information that is presented seems often to be trivial. In a large chapter on politics, for example, we are informed

that there were three organs of public authority with some claims to being representative. We are given one sentence on the *fabriques* (parish vestry councils) and two paragraphs on the *Consulat*. In the thirty-seven pages devoted to the third organ, the *Communauté de ville*, we are overwhelmed with detail on the biographies of its members and on the petty disputes between the *Communauté* and the canons or nobles and how these disputes (almost always over precedence) may have merged with province-kingdom struggle. We are, however, given no real discussion of the powers and duties of the *Communauté*. Part I reads like nothing so much as an attempt to expand a manuscript judged to be too small by an editor. It lacks substance and its 150 pages should have been condensed to one introductory chapter, with no pretense made at dealing seriously with the town.

On the basis of its analyses of the countryside, *Vannes and Its Region* is a fine piece of scholarship that deserves to be read. It makes a real contribution to our understanding of the Old Regime. It should also give those of us who specialize in other regions of France great respect for the bravery of the historian who invades the *bocage*.

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HARVEY CHISICK. *The Limits of Reform in the Enlightenment: Attitudes toward the Education of the Lower Classes in Eighteenth-Century France*. Princeton: Princeton University Press. 1981. Pp. xvi, 324. \$22.50.

This book is lucid and well researched. It is welcome, because the bulk of previous work about French education at the end of the Old Regime has concentrated on *collèges* and universities, in which bourgeois boys predominated. Here Harvey Chisick relates the views of many men of the Enlightenment, some famous and others obscure, about what kind of instruction, if any, the children of the laboring poor should receive.

Great discussion about education began after the removal of the French Jesuits from their schools (1762–64). Those participants who mentioned the lower classes differed about details, but they usually agreed about generalities. Until the early 1770s they opposed schooling for the masses, even though they believed strongly that education was a good thing. Thereafter they said that "the people" should receive instruction "in the three r's and religion, together with a degree of training in subjects of occupational and domestic utility" (p. 181), *not* in Latin or the humanities. The children should have physical training (cold baths, especially) to toughen them for the hard lives facing them. The teachers should also stress patriotism.

Chisick believes that the true significance of the debate about popular education lies within the context of the climate of opinion at the time. Educated Frenchmen saw their country in a crisis, the symptoms of which were depopulation, corruption, and degeneration. They proposed to resolve the crisis through patriotism, *bienfaisance*, and education. They finally had come to acknowledge that the laboring population was important economically. It led a necessarily difficult existence. It may have been more virtuous than the wealthy. But it was still mistrusted and feared. Should it be educated?

Chisick emphasizes that the Enlightenment thinkers were caught in a dilemma, between (1) following their ideology to extend education to everyone in society and (2) giving in to the reality of the "people-condition" ("an economic system requiring endless and brutalizing labor" [p. 279]). The result was a compromise: education according to place in society. Although these men supported the prevailing social and political institutions, they wanted, nevertheless, to reform them gradually. Education could bring about that reform. They did not realize, however, that such education could not resolve the real crisis of the time, which was economic rather than moral.

Chisick states that the study of attitudes about popular education should be extended backward and forward in time. I believe that the Old Regime institutions providing instruction for the lower classes require further definition and study. This is especially true for the many primary (secondary?) schools that emphasized the three r's and religion but were then improperly called *collèges*. Finally, Chisick cites two examples of persons wanting to enlighten the lower classes, not merely to instruct them about useful subjects and religion. I find it curious that those examples fall within the period of the Revolution, which is not the focus of his study and which provides a different context for educational thought. His study in general increases our knowledge of popular education and of the Enlightenment.

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DOMINIQUE JULIA. *Les trois couleurs du tableau noir: La Révolution*. (Fondateurs de l'Éducation.) Paris: Belin. 1981. Pp. 394.

Dominique Julia's colorfully titled work is part of the "Fondateurs de l'Éducation" series, which presents documents from crucial periods in the history of French education within their original context. This, in fact, is exactly what Julia has done. Hence, no one ought to be surprised that this work is a carefully selected and organized compendium

of mostly long quotations by Revolutionary and pre-Revolutionary educational theorists and politicians, as well as the texts of official decrees. While Julia sets the stage for his presentation with a masterful preface describing the five important time periods for the history of public instruction, he remains faithful to his plan by not writing a conclusion. There are also both footnotes and endnotes, marginal notes, illustrations, a chronology, and an index to assist the reader who otherwise might be overwhelmed by the amassed quotations. Julia warns readers that his selections are limited to speeches and printed projects (that is, no archival materials).

The collection's editor, Emmanuel Camille-Berard, is correct when he asserts (p. 7) that the modernity of these documents is astonishing, since Julia's other principle of inclusion was to select those that set the stage for the scholarly debates of the nineteenth century. He treats, in turn, public instruction's *raison d'être*, the debate about the proper role of the state, teacher certification standards, the proper location and number of schools, moral instruction, primary school curriculum and textbooks, the *écoles centrales*, professional technical education, female education, and adult education. By reviewing these specific aspects, he proves his point—made in chapter 10—that Revolutionary educational theory was based on the assumption that the total French society was a "gigantic school" (p. 347).

In chapter 9 and elsewhere, Julia shows that eighteenth-century gender identity and sex role stereotyping was not altered by what the Revolutionaries themselves claimed was a complete break from the previous historical process. In fact, the similarities between theories expounded before and after the Revolution point to considerable continuity, except for the society-schoolhouse notion.

One striking bit of information revealed in passing, in discussions concerning the teaching of language in primary schools, is that Abbé Sicard's methods for teaching the hearing impaired were considered valuable for the light they shed on the learning process of nonhandicapped children (pp. 223–24) instead of vice versa. Experts in special education today might consider this a seminal idea.

All of these "dreams," of course, resulted in minimal action, but they did set the stage for the Napoleonic revolution in education. With the use of these collected documents it should be possible to assess the emperor's "modernity." Julia has performed a service by assembling basic data that heretofore was widely scattered and beyond the reach of American college students.

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ALAN FORREST. *The French Revolution and the Poor*. New York: St. Martin's Press. 1981. Pp. x, 198. \$25.00.

In the last decade historians like Cissie Fairchilds, Jean-Pierre Gutton, and Olwen Hufton have shown renewed interest in the problems of poverty in eighteenth-century France. Yet their studies give little consideration to the impact of the French Revolution on systems of charity. Alan Forrest, in *The French Revolution and the Poor*, fills this gap. He examines the measures adopted by the Revolutionary assemblies for the poor between 1789 and the early years of the Consulate, which saw the end of the Revolutionary experiments and a return to more traditional methods of relief and finance.

Forrest argues that Revolutionary legislators, and particularly the *Comité de Mendicité* of the National Assembly, viewed the alleviation of poverty as a pressing national responsibility. Imbued with a strong sense of altruism and the spirit of inquiry fostered by the Enlightenment, the *Comité de Mendicité* undertook an ambitious investigation to find remedies for the problems of the poor. Its research and reform proposals formed the basis of much of the Revolutionary legislation on the poor between 1790 and 1794. Ultimately, however, insufficient tax revenues, the inability of the bureaucracy to implement legislative decrees, and the exigencies of war undermined the good intentions of the legislators. After 1794 the Thermidorians and the Directory, obsessed with economic problems and the war effort, once again imposed poor relief and its financing on the localities. But, Forrest asserts, the Revolutionary effort should not be dismissed simply because the achievements of the early Revolutionaries fell short of their hopes. Their failing was to pursue an impossible goal—the forging of a national legislative framework in a largely rural and illiterate society that had little sense of itself as a national entity. Indeed, Forrest concludes, “it is astonishing that so much was achieved in such unpromising conditions” (p. 176).

No brief review can do justice to the wealth of information Forrest brings to his arguments. *The French Revolution and the Poor* is informed by research in the municipal and departmental archives of more than a dozen departments and the Archives Nationales. Furthermore, Forrest's analysis of programs for foundlings, the elderly, the indigent, and other groups gives us a closer knowledge of “the poor” as a social category. His recurrent arguments are that state assumption of charity was a logical result of Physiocratic thought and that in many respects Revolutionary change was little more than a continuation of previous reforms, but these judgments may understate the National Assembly's achievement. However antiquated or inefficient Old Regime institutions may have seemed, they had a resiliency that enabled them to withstand most reforming currents, as Maupeou learned in 1774 and as Turgot discovered in 1776. Although the Physiocrats and others had conceived of many of

the reforms before the Revolution, it was the National Assembly that smashed the institutional structure of the Old Regime and made effective state intervention in society possible.

This reservation in no way detracts from the book's value. *The French Revolution and the Poor* is an important contribution to the study of the French Revolution and the literature on poverty in eighteenth-century France.

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JACQUES LÉONARD. *La médecine entre les pouvoirs et les savoirs: Histoire intellectuelle et politique de la médecine française au XIX^e siècle.* (Collection Historique.) Paris: Aubier Montaigne. 1981. Pp. 384.

Jacques Léonard, the leading and most prolific authority on the social history of medicine in nineteenth-century France, here surveys the medical experience in successive political and scientific contexts beginning with the French Revolution and terminating with the Belle Époque. Aimed, it appears, at a general readership, Léonard's coverage is encyclopedic; there can be few movements, events, or personages in nineteenth-century French medicine that he has failed to touch upon in these eighteen chapters. Léonard does not claim to attempt a synthesis but rather to show “the complexity and subtlety of the connections between institutional and intellectual history” (p. 7). Given the rapid review approach, he inevitably does better with institutions than ideas. The clinic, experimental physiology, phrenology, bacteriology, and so on simply do not lend themselves to the brief *aperçu*. At times, the book resembles a popular style of historiography that might be termed “the medical world of . . .” genre. Happily, Léonard counterbalances this tendency toward diffusiveness with sophisticated and critical comments, quantitative illustrations, and, especially, with generous suggestions of promising topics for future research.

The more compelling sections of the book deal with the internal history of the French medical profession. Léonard skillfully leads the reader through a maze of proposals, debates, and laws regulating the profession. He traces the vicissitudes of the elite and the second-class doctors, the *officiers de santé*, finally abolished in 1892, but whose last remnants still practiced on the eve of the Second World War. There are lucid discussions of medical syndicalism (the General Association founded in 1858) and public health activities.

The book is notably weaker on *les savoirs*, where Léonard fails to probe the standard accounts sufficiently to show the links between scientific doctrine

and public power hinted at in his title. One would like to see how he might develop intriguing suggestions that the work of Claude Bernard, Louis Pasteur, and other savants interacted directly with the medical professional movement. Léonard appears on firmer ground when he relates professionalization to the rising prestige of physicians, which in turn stemmed from the demographic and economic consolidation of their clients, the bourgeoisie. Ideological power more than therapeutic efficacy reflected the ascension of *homo medicus*.

Léonard skillfully compares medical structures at Paris with those in provincial centers, and he contrasts urban progress with rural stasis. He steers a judicious middle course between an optimistic Whig interpretation of medical beneficence and the equal but opposite extreme of medical repression by showing that the enterprise was too diverse and too limited to exert such powers in either direction.

This book, replete with names and facts, impressionistic and even pointillistic, reliably conveys what happened to French medicine during the nineteenth century. It seldom pauses long enough on a specific problem to consider how or why knowledge and power altered as they did. The implicit interpretation, though perhaps not intended, is cumulative progress according to a positivistic schema. The book should prove useful to historians as background reading and in the classroom. Unfortunately, there is no subject index to complement the index of names. Endnotes and a list of primary sources, arranged by subject, furnish valuable guides to further exploration. The neglect of secondary works is conspicuous, especially as regards non-French historians.

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RONALD AMINZADE. *Class, Politics, and Early Industrial Capitalism: A Study of Mid-Nineteenth-Century Toulouse, France*. (Studies in European Social History.) Albany: State University of New York Press. 1981. Pp. xx, 334. Cloth \$29.50, paper \$9.95.

Ronald Aminzade traces the connections among industrial capitalism, social class, and politics between 1830 and 1871 in France as a whole and in Toulouse in particular. He centers most of his analysis on Toulouse's working class and its changing cultural institutions, political ideologies, and collective actions.

Aminzade argues that in Toulouse, where handicraft production persisted through the middle decades of the nineteenth century despite the marked growth of industrial capitalism, artisans were the numerically strongest and politically most active

members of the industrial working class. Artisans were most threatened by the growth of new capitalist forms of industrial production and maintained traditions and institutions that enabled them to recognize this threat and mobilize against it. During the early years of the July Monarchy they did this by holding on to traditional artisan associations, relying on aristocratic and Catholic sources for economic and social support, and looking to popular royalism for political strength against the bourgeois regime of the Orleanists. During the latter years of the July Monarchy and the Second Republic, artisans turned to secularized institutions such as mutual aid societies, allied with radical members of the bourgeoisie who claimed to represent their interests, and adopted new political ideologies such as Icarian communism and above all republican socialism. Underlying all this was the growth of industrial capitalism, which meant the increasing proletarianization of the working class, the decline of handicraft modes of production, the decline of aristocratic wealth, and the rise of the industrial bourgeoisie. Politically this was manifested in the changing electoral alliances, the class struggles, and the revolutionary upheavals of the period.

Aminzade concludes that prior to 1871 working-class political and cultural opposition, led by artisans, undermined the efforts of the bourgeoisie to establish their hegemony but ultimately failed to produce an effective counter-hegemonic force. After 1871 the French bourgeoisie gained full hegemony thanks to the continuing decline of handicraft production, the further development of industrial capitalism, the failure of working-class opposition politically to go beyond local and urban boundaries, and the ability of the bourgeoisie to develop a state apparatus that appeared to be neutral.

This is an excellent book. Aminzade's focus on Toulouse and the French republicans gives his arguments a concrete quality, while the connections he uncovers between politics, class, cultural institutions, and economic developments give this book importance far beyond the usual local study. Aminzade makes sophisticated use of Marxist theory to structure and enhance his analysis. His arguments are well supported by sources ranging from traditional archival documents to police records, voting lists, and census reports.

There are, however, some problems. In the preface Aminzade does not clearly state the main purposes of the book. In the middle chapters his political narrative becomes so detailed as to lessen the impact and appeal of what he is arguing. In the otherwise well-thought-out conclusion he fails to make the connections to Toulouse that generally hold the book together so well. But these problems are irritants rather than major flaws. Aminzade's book is an insightful and important contribution to

our understanding of nineteenth-century French history.

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THOMAS R. FORSTENZER. *French Provincial Police and the Fall of the Second Republic: Social Fear and Counterrevolution*. Princeton: Princeton University Press. 1981. Pp. xxi, 336. \$25.00.

According to Thomas R. Forstenzer, all previous historians of the struggle between France's reviving conservatives and the red republicans of the Moun-tain that climaxed with the wave of repression after the coup d'état of December 2, 1851, have been led astray by a fundamental question *mal posée*. From Marx to Merriman, all have assumed that the left was effectively crushed by June of 1848 or 1849 and that subsequent conservative assertions that there was still a social danger were but a hypocritical hoax intended to justify resort to authoritarianism—"a public relations pitch of the Élysée."

Whether he agrees or not with this generalization, a reviewer might expect Forstenzer to investigate how real the continuing menace was, as Ted W. Margadant, in a more thorough and persuasive study than this one, has recently done. Forstenzer insistently reiterates, however, that effective revision requires seeing counterrevolution not as a "mere corollary of revolution" but as "a historical process" with "a logic, mystique, and program of its own." His investigation therefore focuses on "the continental bourgeoisie that Michels and then Gramsci would later study" in action, examining the outlook, objectives, and apprehensions of the prefects and *procureurs-généraux* of the Second Republic. Unfortunately, despite this portentous invocation of novelty, the conclusions that emerge from Forstenzer's curiously misnamed "samples" of departments, their personnel, and the records of their *Commissions mixtes*, though sometimes suggestive, are basically quite unsurprising. They thus make one wonder if the author's own *question*, if not *mal posée*, is perhaps *passée*.

After a background chapter that tells us that "the bourgeoisie, the beneficiary of Louis-Philippe's regime, was itself a class in transition," Forstenzer focuses on the careers of the prefects and *procureurs* of the seven (of 80 odd) departments he has selected as representative. He finds among them and among the rest of the French "power elite" of the 1840s the intricate relationships of kinship and patronage that Beau de Loménie and Lhomme, neither cited, have led us to expect. Next he analyzes their reports, suggesting that their language reveals both that the

red menace was real and that the legal machinery of the Republic could not control it (partly because "bourgeois" judges insisted upon evidence for convictions). Surely, however, the extent of the menace, as distinguished from the authorities' perception of it, cannot be gauged from reports based, according to Forstenzer himself, on inadequate police information. Similarly, his assertion that the reports' objectivity belies any conservative campaign to exaggerate social panic is rather weakened by the fact that these assessments were not intended for publication. Not everyone will be persuaded by the author's contention that efforts at repression were largely ineffective before the coup. After all, almost a third of the Republican papers in his seven departments were out of business before December 2. One can hardly disagree, however, with his characterization of its aftermath as an attempt at a "final solution" of the "démocratie" problem, meting out more savage penalties to middle-class radicals whose only offense was to be social renegades than to peasants captured with weapons in hand.

Need we perceive in this, as Forstenzer insists, the climax of "a radical dynamic" of "total social prophylaxis?" This intensively if unevenly researched and argued book does offer historians some useful cautionary admonitions against the recurrent assumption of bourgeois guile and hypocrisy. We shall never know precisely how much French middle-class conservatives had to fear from 1852, for whatever conspiracies were preparing for it were aborted in 1851. The fact that he cannot answer this either does not bother Forstenzer, for whom it is the wrong question; fear may be genuine even when it is unrealistic, as we need not necessarily assume it was. This is a useful reminder. Perhaps historians do not always remember that sentimentally we, too, are on one side or the other of the barricades, and not always now on the side where our profession and social status would naturally have placed us in 1851.

Ironically, however, Forstenzer has himself fallen into an interpretive trap not unlike the one in which he sees his predecessors ensnared. For if, as he argues, French middle-class conservatives saw themselves faced with the literal prospect of anarchy and ruin in 1852, we can hardly react with surprise, let alone with Forstenzer's barely suppressed indignation, to the ferocity with which they exploited December 2. Nor do we need to overdetermine their actions by positing premeditation since 1848 or "a radical dynamic" with "a logic, mystique, and program of its own." It is not only the revolutionaries of this world, after all, who can simply claim self-preservation as the ultimate justification for a resort to force. But if that is the chief lesson of this book, it is not, alas, a very novel one.

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JAMES F. MCMILLAN. *Housewife or Harlot: The Place of Women in French Society, 1870–1940*. New York: St. Martin's Press. 1981. Pp. 229. \$25.00.

Despite its inauspicious beginning with a sensationalist title and a curious misstatement that "women, until recently hidden from history, are currently faced more with overexposure than with neglect," James F. McMillan has enriched the historiography of France by publishing the first book-length overview of women in the Third Republic in half a century. As a source of information about attitudes, customs, laws, and institutions affecting women, about leading individuals and organizations, about women at home and women at work, the book provides a welcome complement to standard works that ignore the female experience. Although marred occasionally by misleading interpretations (French feminism as a "purity crusade") and trivializing terms ("clerkess," "manageress," "negress," "girls" meaning "women"), it will serve well as a supplementary text until a more perceptive analysis comes along. Ultimately, however, the value of this book is reduced by its outdated conceptual framework.

McMillan reveals the limits of his vision by declaring at the outset that "the most fundamental question . . . is the extent to which [women] have been relegated to a position of inferiority on account of their sex." That he subscribes to the "woman as victim" school of historiography is indeed implicit in his use of Proudhon's infamous dictum as his title. In examining the meaning for women of the doctrine of separate spheres and the double standard of morality, he justly criticizes facile interpretations of nineteenth-century family life and women's roles. Yet he adds little that is new on female participation and power in the private sphere. Instead, misled by traditional periodization, he selects as the primary analytical focus of the book the "important question of whether or not the First World War represented a decisive stage in the process of female emancipation in France." But shooting down historians who uphold this 1920s perspective is all too easy and not very interesting. Since the resurgence of women's history a decade ago, it is impossible to take seriously vintage arguments that women were "emancipated" when some of them got the vote and others cut their hair.

Conceptualization aside, McMillan demonstrates convincingly that throughout the period under consideration, "domestic ideology" persisted despite efforts toward educational reform, relatively high female labor force participation, feminism, and, finally, the disruptions of war. The most original and valuable part of the book is the description of female experience on the home front, especially that of working-class women. As household managers;

as wives, mothers, and lovers of soldiers; or as women called to do "men's work" in fields and factories, they made sacrifices disproportionate to rewards gained. If McMillan's account of massive strike activity by garment workers in 1917 shatters the myth of female docility in labor disputes, his description of men's response to women's wartime work reinforces allegations of syndicalist antifeminism. Trade unionists saw that "it was female labour which allowed male comrades to be sent off to the slaughter in the trenches." They also worried that women doing men's work for women's wages would permanently depress men's earning power; hence the law of July 10, 1915, which established the first minimum wage for industrial homeworkers. In the end, McMillan believes, by destroying men and mobilizing women for paid work, the war exacerbated antifeminist tendencies. At the end of hostilities, women were demobilized. Government men offered "redundancy payments" to encourage women to quit work voluntarily; businessmen closed the better jobs to them; union men established antifemale quotas. Government, industry and trade unions, politicians, physicians, and priests united toward the "consolidation of domesticity."

What is missing from this account, and renders the focus on war unsatisfying, is a framework that relates the lives of women, bourgeois and working class, to long-range, fundamental political, religious, and economic issues and institutions. The book is marked also by a conspicuous absence of references to many recent contributions by American historians of French women, as well as more generally to the American school of women's history, which has led the field across disciplinary barriers to conceptual and methodological advances. Since McMillan's bibliography lists only unpublished, primary sources, a reference to Karen M. Offen's bibliographical essay (in *Third Republic/Troisième République* 1 [1977]: 238–99) would have been helpful. Clearly more research on women's experiences in production and reproduction, public and private life is necessary before a successful synthesis can be achieved.

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MAURICE AGULHON. *Marianne into Battle: Republican Imagery and Symbolism in France, 1789–1880*. Translated by JANET LLOYD. New York: Cambridge University Press or Éditions de la Maison des Sciences de l'Homme, Paris. 1981. Pp. viii, 235. Cloth \$39.50, paper \$14.95.

La Rochefoucauld once noted that gifts express the interests of the giver more than they do the desires

of the recipient. This maxim is certainly true for nations if not for individuals. How else can the historian explain the 1886 French gift of a female Statue of Liberty to a virile United States? Ostensibly celebrating Franco-American friendship, this public monument in New York harbor actually personifies what the donors wished to see in France after 100 years of searching for a stable regime: a distinguished Marianne no longer troubled by revolution or reaction.

The origins of this image are well traced by Maurice Agulhon's *Marianne into Battle*, which is in effect an extended introduction to his forthcoming quantitative study of Third Republic statuary. But the present essay on statues, coins, medals, stamps, paintings, caricatures, *fêtes*, processions, and folklore, all portraying political ideals as a woman, suggests more fully the historical linkages between the images and the ideas behind them from 1789 to 1880. In this period the Republic was hardly an actuality and its imagery far from established. Although the tradition of presenting the revolutionary Republic of 1793 as Liberty in a Phrygian cap had been interrupted by empire and constitutional monarchy, it was rediscovered and charged with new meaning during the Second Republic. It was then that the Republic was first called Marianne, a popular name designating a popular regime, either derisively or sincerely depending upon one's political disposition. By 1880, however, after republican secret societies had preserved both the image and its name during the Second Empire, the conservative Third Republic adopted a politically less threatening Marianne whose reigning serenity now replaced revolutionary ardor. In short, the image passed from a symbol of the Republic to that of France. In triumphing, the Republic and its imagery—both public and private, elite and popular—changed to suit the new political context.

For the specialist this popularization of more scholarly work in progress is tantalizing, both as a cultural-cum-political history and as a theoretical statement of methodology. Despite its remarkable variety of sources, including literary, artistic, and archival material, *Marianne into Battle* does not yet fulfill the promise of the author's *Annales* article in 1973. The book's provocative range of argument and documentation only makes the more fully developed companion volume that much more eagerly awaited. Nevertheless, what the present work does on its own is provide a bold overview of popular ideals in nineteenth-century French politics. The chapters on the Second Republic document conclusively, I think, the independent conception of the Republic developed by secret societies in the South that their counterparts in the North later incorporated into their own definition. Moreover, Agulhon shows how the survival—indeed, the spread—of this

symbolism during the Second Empire laid the popular foundations of the Third Republic and its official imagery. Consequently, Agulhon's wider audience in America can now understand the broad historical significance, for France at least, of Bartholdi's Statue of Liberty: not a Jeanne d'Arc but a Marianne, crowned by sunbeams instead of a Phrygian cap.

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GERD KRUMEICH. *Aufrüstung und Innenpolitik in Frankreich vor dem Ersten Weltkrieg: Die Einführung der Dreijährigen Dienstpflicht, 1913–1914*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, number 96.) Wiesbaden: Franz Steiner. 1980. Pp. vi, 297. DM 68.

This book is mistitled. It is not a political-economic study of French rearmament but an account of the political fight in France over the reintroduction of three-year obligatory military service in 1913. It is also, Gerd Krumeich emphasizes, a revisionist work, consciously setting out to rescue the three-year law from a historiography of socioeconomic extrapolations. This historiography (including works by Eugen Weber, Arno Mayer, G. Michon, Madeleine Ribérious, and others), stressing the *Primat der Innenpolitik*, presents a scenario of polarized political struggle between a pacifist, internationalist, collectivist left (opposing three-year service because it would create a large, socially reactionary, active army) and a stridently patriotic, fiscally conservative, noncollectivist right (favoring the three-year law for the same reason).

Krumeich rejects this scenario for three reasons. First, he argues that the left (the SFIO and the CGT) opposed the three-year law on socio-ideological and military rather than pacifistic and internationalist grounds. Consequently, although it differed with the right about the composition and social role of the army, it did not differ with it—as the left's enthusiastic acceptance of the *union sacrée* in 1914 indicates—about the need for national defense. Secondly, Krumeich contends that the Radical Republicans approved the three-year law for military, not social, reasons. Specifically, they accepted the view that the active army had to be enlarged (by three-year service) in order to repel an *attaque brusquée* from Germany (something a reserve army, because of the mobilization time factor, could not do). Thirdly, the socialists' belief (and that of subsequent historians) that the Poincaré-Barthou government wanted the three-year law for domestic political reasons is incorrect. Krumeich argues, quite convincingly, that the government acted primarily

from military-diplomatic motives. Convinced after the Agadir crisis of 1911 that war with Germany was inevitable and equally convinced that victory could be gained only by taking the offensive, the general staff (under Joffre) drafted Plan XVII. Because success also depended on a coordinated attack by Russia, Plan XVII became closely tied to the Russian alliance. And since the Germans increased the size of their army in 1913, the French needed a three-year draft in order to have an active army of sufficient size to carry out offensive operations. The three-year draft proposal was not, then, the government's reaction to radicalism in France but its response to the Reichstag's army bill of 1913.

Krumeich does not argue, however, that domestic political factors did not enter, in fateful ways, into the calculations of the protagonists. He claims, to cite the most obvious example, that Poincaré and Joffre sought war in July 1914 because they feared that the recently elected, leftist-dominated Parliament, by repealing the three-year law, would compromise Plan XVII; they felt that since war was inevitable, it was better that it come sooner than later. But Krumeich rejects Arno Mayer's contention that Poincaré wanted war in order to shatter the 1914 Socialist-Radical electoral coalition.

By staying close to the documents, Krumeich is able to question treatments of the three-year law that make it more a matter of domestic partisan politics than national defense. The book, therefore, calls upon historians to re-examine the complex issue of cause and effect when considering the march of these fateful events. It is an excellent monograph that deserves to be read in English- and French- as well as in German-speaking scholarly circles.

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W. D. HALLS. *The Youth of Vichy France*. New York: Clarendon Press of Oxford University Press. 1981. Pp. xi, 492. \$59.00.

Before the late nineteenth century, states largely ignored youths for any purposes other than military. Indeed, adolescence as a distinct stage in human development seems to have become apparent only recently, in those highly differentiated industrial societies where a gap opened up between childhood dependence and adult employment. First came the late nineteenth-century extension and universalization of secondary education. In this century governments tried to organize and indoctrinate youth, while oppositions, labor movements, and churches did the same. Third Republic France, with its weak associative life, lagged in this enterprise.

Scouting and youth hosteling, for example, began to take root in France only in the 1930s, a generation later than in Britain and Germany. In 1939, W. D. Halls tells us, only 15 percent of French youth participated in the various nationalist, socialist, Christian, or scouting associations.

The German occupation of 1940–44 was a time of catching up in this respect. Halls has been the first to consult both central and local archives dealing with the Vichy regime's educational policies and youth organizations. He has given us by far the richest account to date of these programs and their doctrines.

Halls gives us very little, by contrast, of the broader context sketched at the opening of this review. While he adheres to current scholarship in regarding Vichy youth policy as indigenous rather than a German imposition, he does not explore its roots in the quarrels and conflicts of the Third Republic. Did Vichy's urge to mobilize the young derive more from a practical fear of disorder, from competition with socialist youth organizations, or from earlier obsessions of the French right with natality and family nurture? Nor does Halls compare Vichy's efforts and results with those of either democratic or totalitarian states.

Compared to Germany, Italy, or the Soviet Union, of course, Vichy's youth programs remained heterogeneous without being really pluralist. Statists and decentralizers, Catholics, nationalists, and collaborators fought for hegemony over French youth, and no one official movement emerged to give institutional drive to what was, in fact, a quite homogeneous set of ideas. Halls's other main conclusion is Vichy's failure to impart its hierarchical and corporatist moralism to French young people. He proves his point with a fascinating but probably irrelevant discussion of crime and antisocial attitudes among wartime French youth. Did any regime successfully indoctrinate more than a *minorité agissante*, however? Here again, comparison would have given this admirable description a sharper analytical edge.

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RICHARD F. KUISEL. *Capitalism and the State in Modern France: Renovation and Economic Management in the Twentieth Century*. New York: Cambridge University Press. 1981. Pp. xiv, 344. \$37.50.

In 1943, one Daniel Villey labeled the participants in an ongoing debate about planning and economic management either "architects" or "doctors." The first were primarily concerned with the structure, the latter with the content of economic activity. Richard F. Kuise, from whose book the Villey refer-

ence is drawn (pp. 158–59), reveals himself much more the architect than the doctor. His treatment of change in the French system of political economy emphasizes people, plans, policies, and commentary while relegating the history of the economy itself to the role of context. Like architectural historians, Kuisel is almost as interested in projects that were never put into practice as in those that were. Particularly in the early chapters, the parallel discussions of ongoing policies and events on the one hand and visionary thinking on the other do not manage to avoid a certain tinge of unreality. The pace picks up when the narrative reaches the Popular Front, whose history is all too timely in these days of early Mitterrandism. The genesis of planning in both camps during the long night of occupation or exile is also well handled.

Kuisel's thesis is not readily distinguishable from other scholarly analyses of the French case that he cites. The moving force behind change in economic structure and performance was a sense of backwardness, ripening into the will to "renovate" or modernize. One way the author would distinguish his approach from that of bloodless quantifiers is by his stress on leaders, from precursors such as Étienne Clémentel to Jean Monnet himself (neither rates a street in Paris as yet). But his main effort, and accomplishment, is that of providing detailed documentation for a generally accepted interpretation. Although there is a good deal of attention paid to various "systemic" currents of thought—notably liberalism, corporatism, and socialism—the arguments regarding their influence are often fudged. Liberals were leaders in the movement toward active state management of the economy after 1945. France was liberal before 1914, but its liberalism was distinctively state tainted. The new postwar economic order had strong corporatist elements, but these were in fact only the usual lobbies and pressure groups. Concern for social justice was one motive behind planning, but the emphasis in public spending plans was on investments in economic infrastructure.

Despite my admiration for the material in the book and for the skill shown in organizing and presenting it, I am left with some uneasiness regarding substance and a couple of complaints on matters of form. As a traditional historian, Kuisel is tied fast to his documentary sources. Yet those who spoke or wrote abundantly could have been misinformed or engaged in axe grinding. On the other hand, others may well have exercised power or influence in silence. One can remain skeptical about conspiratorial French views regarding control of the economy by a clique, whether of 200 families or of *grandes écoles/grands corps* technocrats, but here they seem to be dismissed principally because the potential conspirators left no programmatic statements. As regards form, while the inconsistent and untidy han-

dling of French terms is a minor irritant, the absence of a bibliography is a major flaw. This book stands on its sources, and the short bibliographic notice will not repay the reader for the agony of tracking them down among more than 900 footnotes.

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MICHAEL M. HARRISON. *The Reluctant Ally: France and Atlantic Security*. Baltimore: Johns Hopkins University Press. 1981. Pp. xiii, 304. \$24.00.

The fascination with Charles de Gaulle continues even in this post-Gaullist era. Indeed, now that a decade and more has elapsed since de Gaulle's departure from the Élysée, an evaluation of his enduring legacy in the realm of foreign affairs has become more feasible. In *The Reluctant Ally: France and Atlantic Security*, Michael M. Harrison attempts such an evaluation, focusing on de Gaulle's response to the NATO alliance in the perspective of the periods before and after de Gaulle's administration. Harrison's lucid analysis, which will interest both students of contemporary American and French foreign policy, pays serious attention to Gaullist ideas in the area of policy and attempts to demystify them. Harrison argues convincingly that once we strip away the rhetorical trimmings, the Gaullist vision was both a reasonable and an appropriate one for France and, in the long run, desirable for NATO. Moreover, despite appearances to the contrary, there was a substantial continuity between Fourth and Fifth Republic Atlantic alliance policy. To be sure, de Gaulle was more successful because of the greater stability of his regime. Nonetheless, as Harrison suggests, de Gaulle's success was based in large measure on his ability to articulate views that were widely shared by Frenchmen of all political persuasions.

More specifically, Harrison shows that the aims of the leaders of the Fourth Republic and later de Gaulle were virtually identical: to protect France against external threats, to give the country access to modern weapons, and to achieve equality with Britain and military superiority over the Germans. Fourth Republic leaders entered the alliance with the hope of achieving these goals. They were as persistent, though less successful, in pursuing them as was de Gaulle. Even as the Fourth Republic was about to fall, Premier Gaillard refused to accept American nuclear weapons on French soil. De Gaulle brought greater clout to this struggle and eventually removed France from the integrated military forces under NATO, but Harrison is at

pains to show that de Gaulle remained committed to the Atlantic alliance. As late as 1968, he began a modest rapprochement with it. Hence, the improved relations between France and the U.S., normally attributed to Pompidou and Giscard, were actually launched by de Gaulle.

Although political scientists will applaud this "long view" of de Gaulle's policy, the historian must question the absence of any references to the pre-war period, when the problem of French inferiority to American power first emerged. Some attention to the work of Stephen Schuker, Melvyn Leffler, and Charles Maier would have been helpful in this regard. Aside from excessive repetition, Harrison's work also suffers from the failure to develop more fully the insight that de Gaulle's ability to challenge the alliance may well have derived not only from the détente of the 1960s but also from the relative prosperity of France. France could take the risk of challenging the American giant during a period in which it felt strong and self-confident at home.

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WILLIAM A. CHRISTIAN, JR. *Apparitions in Late Medieval and Renaissance Spain*. Princeton: Princeton University Press. 1981. Pp. vi, 349. \$25.00.

William A. Christian, Jr., is that rare scholar whose training in social anthropology assists rather than obscures his historical research. He has already published *Person and God in a Spanish Village* (1972) and *Local Religion in Sixteenth-Century Spain* (1981). The latter book was based on the replies to the questionnaire addressed by Philip II in the 1570s to the towns and villages of New Castile. There and in the present book Christian is concerned not with the institutions of the state church of Habsburg Spain but with "popular religion," the personal contact between the people of Spain and their patron saints.

In *Apparitions* he casts his net wider than in *Local Religion* and investigates "a series of reports of celestial visions of common people, children, farmers, shepherds' wives, servants." The author's approach to these visions is clearly stated: "Rather than explaining away the visions, or even explaining them, I have tried to learn from them how people experienced both the world they knew and the world they had to imagine" (p. 9). He has certainly learned much. His book throws light on commonly neglected aspects of Spanish society (such as its attitude toward children) as well as on the religion of the time (for instance, the increased insistence on penance and the Passion).

The basis of the book is a series of texts in Castilian and Catalan that purport to record visions ranging in date from 1399 to 1523 (with two later

cases); the texts we have are often considerably later than the visions they record. These texts—taken from a variety of local archives as well as from the Archivo Histórico Nacional in Madrid—are printed, mostly for the first time, in the appendix (pp. 245–331). The author has translated them, in slightly abbreviated form, in the body of the book and has set the stories they relate in their historical context.

Most of the stories of apparitions here collected concern the Virgin Mary, although a few other saints also appear (Anthony of Padua, Michael the Archangel, Sebastian, Roch). The stories told are clearly interrelated, but, as Christian observes, it is not a matter of literary but of "real life imitation" of earlier celebrated happenings, made known by letters, sermons, and the circulation of questors collecting alms for established shrines (p. 210).

In Catalonia, unlike Castile, the visions recorded were all connected with contemporary outbreaks of the plague. The Catalan visions also shared an emphasis on moral reform lacking in Castile; this Christian reasonably connects with the slightly earlier sermons of St. Vincent Ferrer. Catalan and Castilian apparitions almost all took place in rural settings. The apparitions assisted the reestablishment of earlier rural Christian shrines after a troubled period of plagues and wars (p. 207).

Christian includes two early sixteenth-century accounts of cases where the Inquisition of Cuenca repressed local visionaries closely resembling the "seers" of visions accepted as authentic some decades earlier. One may compare the attitude of the Inquisition here to its equally skeptical approach to mystics in general, including some who became accepted as leading saints; see, for instance, Enrique Llamas Martínez's, *Santa Teresa de Jesús y la inquisición española* (1972).

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WILLIAM J. CALLAHAN. *La Santa y Real Hermandad del Refugio y Piedad de Madrid, 1618–1832*. (Biblioteca de Estudios Madrileños, number 25.) Madrid: Instituto de Estudios Madrileños. 1980. Pp. 186.

The analysis of charity and poor relief in early modern Europe has come into vogue in recent years, not only for its intrinsic importance but also for what it can teach us about the intellectual and philosophical bases of Old Regime society. William J. Callahan of the University of Toronto has made a useful contribution to this growing field with his study of the most important charitable brotherhood in Madrid during Habsburg and Bourbon times. Madrid experienced tumultuous growth in the early seventeenth century, attracting emigrants

from all over Spain and the rest of Europe. By 1630 it had grown to perhaps one hundred seventy thousand persons, and, depending on how we define poverty, some 10 to 20 percent of them needed help to survive at any given time. The brotherhood generally known as the *Refugio* sprang up to address some of that need. Growing from a small informal prayer group in 1615, the *Refugio* was chartered in 1618 and began attracting membership and donations. Its members were nearly all drawn from the lay and ecclesiastical nobles resident in Madrid, but, given the wide range of fortunes among the Spanish nobility, the *Refugio* reflected diversity as much as unity. The most important unifying factor was its members' adherence to the religious injunction to give alms and to aid the poor as part of their identity as privileged members of society. The minor nobles, especially, seemed anxious to accept their charitable responsibilities, in part at least to affirm their status as nobles. The poor were supposed to respond with gratitude and to accept the social structure as it was.

The *Refugio* members had little thought that their works would end poverty or even reduce its incidence. Instead, they aimed to help the working poor in times of illness and other personal crises, aiding decent, respectable individuals to regain their productive roles in society. The chronically indigent, criminals, and prostitutes were generally beyond their purview. The main activities of *Refugio* members—home visits to give alms, nightly rounds to collect derelicts, and ambulance service to take the sick poor to hospital—all aimed at their focused and limited goals. Their only long-term commitment was to found a school for orphaned girls in 1651, which was separately funded.

The finances of the *Refugio* generally followed economic trends, with the steady income from rental property and interest on credit paper taking up the slack when cash donations declined in times of crisis. Fixed expenses took an increasing proportion of the *Refugio's* income from the late seventeenth century on, so that by 1804 only 44 percent could be spent on direct charity. Nonetheless, the *Refugio* managed to survive economic and political crises through the seventeenth and eighteenth centuries, but in the process its organization became more rigid and its goals even more limited. Disputes between the aristocratic governing committee and the lower-noble bulk of the membership coincided with financial distress in the late eighteenth century, and the upper-class poor claimed more and more of the *Refugio's* resources. In the nineteenth century, the rise of secularism in Spain undercut the *Refugio's* finances by forcing the sale of its properties and destroyed the religious rationale behind the ideals of elite charity and lower-class docility.

Callahan relies heavily on the records of the *Refugio*, supplementing them with several other

archival collections in Madrid and with a selection of contemporary sources and modern secondary works. His background chapter on the Spanish economy is sometimes oversimplified and confused; in his concern to establish the desperate condition of Madrid's poor throughout the period, he neglects to show clearly the changes over time in the national economy. Overall, however, the book is a welcome addition to the study of poor relief and social control in early modern Madrid.

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ÁNGEL VIÑAS. *Los pactos secretos de Franco con Estados Unidos: Bases, ayuda económica, recortes de soberanía.* (Colección 80.) Barcelona: Grijalbo. 1981. Pp. 333.

Ángel Viñas, who has enjoyed success as a historian, a professor of economics, and a diplomat, is one of Spain's leading authorities on the politics and economics of the Franco period. His present monograph sets out to analyze the antecedent negotiations and the contents of the 1953 Pact of Madrid, the agreement that provides the foundation for the contemporary relationship between the United States and Spain. Viñas makes no secret of his dislike for the terms of the original document, which included a technical agreement, four annexes, two secret notes, and twenty-two procedural accords. The secret notes Viñas purports to analyze publicly for the first time. The general thesis of this study is that in the 1953 pact Franco derogated sovereignty from his country and subordinated the security interests of the Spanish nation to those of the United States for marginal amounts of economic and military aid. From Franco's point of view, however, it was the not insignificant propaganda value of an alliance with the United States that enabled him to preserve and reinforce the peculiar set of political institutions he had imposed on the Spanish people.

That Franco and his team of negotiators were highly motivated to procure the original agreement is not in doubt. How highly motivated? U.S. public documents indicate that Franco authorized the payment (over a fifteen-year period) of approximately \$750,000 to a well-connected Washington lawyer-lobbyist, Charles P. Clark, for his activation and direction of the "Spanish lobby." The lobby was instrumental in galvanizing U.S. congressional sentiment behind the 1953 pact. What truly motivated Franco and his military to accept the particular terms that Viñas calls "unsatisfactory and unequal," one cannot adduce from the evidence proffered. Thus, as an exposé of a failed Franquist policy, Viñas relies on a partial opening of Spanish archival

sources, primarily those of the Jefatura del Estado and the Ministry of Foreign Affairs. Basically the 1953 agreement was desired by Franco and his military establishment, and they negotiated the document from the Spanish side. Viñas claims that Spanish diplomats were kept in the dark. In any case, the historical record will not be complete until the military archives, currently sealed, are finally made accessible to scholars.

In some of its parts the study appears written as a "court history," but in all fairness Viñas formally announces his biases in the introduction when he states that one of his purposes is that of assisting the crown and the young Spanish democracy in a fuller understanding of the foreign policy of the Franco regime. In sum, Viñas has called our attention to an extremely important dimension of Spain's recent diplomatic history, one to which scholars ought to pay more heed. It is a dimension that continues to affect the country's current security predicament, since the U.S.-Spanish Bases Agreement, upgraded to the status of treaty in 1976, is scheduled for renegotiation in 1982. This is an interesting book for the specialist, because it utilizes tantalizing archival material for the first time and is based on an extensive bibliography of secondary literature. The more general reader will find the study smoothly written and effectively organized, so that a complicated epoch in Spain's recent history becomes comprehensible, albeit in terms that do more to illuminate contemporary Spanish thinking on the subject than the thought processes and strategies of those Spaniards who actually negotiated the original document.

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C. S. M. RADEMAKER. *Life and Work of Gerardus Joannes Vossius, 1577-1649*. Translated by H. P. DOEZEMA. (Respublica Literaria Neerlandica, number 5.) Assen, The Netherlands: Van Gorcum; distributed by Humanities Press, Atlantic Highlands, N.J. 1981. Pp. xxvii, 462. \$48.50.

In his own time Gerardus Vossius enjoyed a reputation as one of Holland's leading humanists. Through his teaching at Dordrecht, Leiden, and Amsterdam's new Athenaeum; by his international correspondence; and because of his many writings on theology, philosophy, and history, he was respected as one of the most influential scholars of the early seventeenth century. His publications included a mammoth treatise on the Pelagian controversy, authoritative studies of Latin grammar and etymology, commentaries on classical historians, several church histories, and numerous theological tracts. Joost van den Vondel, Hugo Grotius, Constantijn

Huygens, Pierre Gassendi, and William Laud were his friends, and he was recognized as a leader of the moderate Remonstrant party. He twice declined invitations to teach at Cambridge but accepted an appointment as a nonresident canon at Canterbury Cathedral. It is surprising, therefore, that despite many commemorative orations, official tributes, and specialized articles Vossius was omitted from the magisterial *Nieuw Nederlandsch Biographisch Woordenboek* and that he entirely escaped full biographical treatment for three centuries.

C. S. M. Rademaker's new book, based on his 1967 dissertation at the University of Nijmegen, is an attempt to remedy that oversight. Researchers should approach this biography, however, fully warned by its title: this is a study of Vossius's "life" and "work," not his personality or historical significance. References are, indeed, made to important happenings and issues (for example, to the Synod of Dordrecht, the Holland School Act, Laud's and Charles I's executions, and the reasons for Vossius's later obscurity). But the author's overwhelming emphasis is on detail, some of it extraneous (for example, the fact that a fire and storm destroyed nine hundred homes, three churches, and the Roermond city hall in July 1554 when Gerardus's father was five years old), some of it taxing (the lengthy doctrinal disputes among the clergy and academics), some of it interesting (descriptions of student life and of Vossius's own library). Readers should not expect incisive analyses, psychological insights, provocative interpretations, or theoretical generalizations. Instead, they must be content with 354 pages of solid text, 1,081 consecutively numbered footnotes, 2 genealogical appendixes, the 8-page Latin text of Vossius's autobiography, 4 bibliographies, and a personal name index. The author obviously intends to provide trees of information, not a forest of wisdom.

On these terms Rademaker's book is an important contribution. It will serve as a straightforward, fair, extensive, factual encyclopedia of Vossius and Vossiana, of data concerning everyone with whom Vossius was related or associated, and of nearly every event that occurred in Holland's intellectual circles during Vossius's lifetime. Although the style is sometimes awkward and pedantic (it is hard to tell if this is partly the fault of the American translator, H. P. Doezema), it is usually clear. The seventy-six page bibliographical apparatus listing Vossius's publications, manuscripts, letters, and secondary works is especially impressive and will prove extremely valuable. Recognizing its limitations, large academic libraries will want to buy this book, and all scholars interested in seventeenth-century European intellectual history will want to know about it.

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M. NUYTTENS. *Camille Looten, 1855–1941: Priester, wetenschapsman en Frans-Vlaams regionalist* [Camille Looten, 1855–1941: Priest, Scholar, and French-Flemish Regionalist]. Summary in French. (Symbolae, Facultatis Litterarum et Philosophiae Lovaniensis, series A, number 9.) Louvain: Universitaire Pers Leuven. 1981. Pp. 268. 980 F.

This is the first biographical study of Camille Looten, French-Flemish regionalist and liberal Catholic clergyman during the Third Republic. It is based on very extensive archival and printed sources and is the product of careful and thorough research. Looten was the son of middle-class parents born in French Flanders. He attended the Catholic seminary at Courtrai and became a priest in 1880. Ten years later he became a professor of English literature at the University of Lille. Looten was one of the few Catholic clergymen who supported Pope Leo XIII's *raillement* and the Separation Laws of 1905. His liberalism was largely the result of the influence of his friend, the Catholic priest Jules Lemire. Interestingly, Looten later accepted the Vichy regime because of its respect for the family, agrarian life, and corporatism. As a scholar and student, Looten's interest was not in the area of Netherlandic literature and culture but in English letters. Much of his energy was devoted to the preservation and resurrection of Flemish culture and language in France. Around the turn of the century the Flemish tongue in France was almost extinct. Only the small *Comité Flamand de France* tried to promote interest in the cultural origins of the local Flemish population. Looten joined this group and successfully served as its president for thirty years.

M. Nuytens could have placed the *Comité* more in the general context of the Flemish movement in the nineteenth and twentieth centuries. Thus we should have been told more about the contacts between Belgian and Flemish regionalists and the Netherlands. Instead, I find too much of a chronological survey of Looten's activities in the *Comité*. Furthermore, I would like to know more about the success of the *Comité*. Did it succeed in reviving an interest in Flemish culture among the natives?

This biography is a bit too factual and somewhat short on interpretation, although a good conclusion at the end repairs this in part. Yet it does give us a good "life and times" of Looten the liberal, Catholic humanist, and regionalist. This biography comes at a propitious time when we see a resurgence of regionalism in Europe.

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L. DE JONG. *Het Koninkrijk der Nederlanden in de Tweede Wereldoorlog* [The Kingdom of the Nether-

lands in the Second World War]. Volume 10a, *Het laatste jaar I* [The Last Year, pt. 1]. In two parts. The Hague: Martinus Nijhoff, for the Rijksinstituut voor Oorlogsdocumentatie, Amsterdam. 1980. Pp. viii, 531; vi, 533–1100.

The tenth volume of Louis de Jong's "official" history of the Netherlands during the Second World War covers the last year of the war, starting with the Allied landing in Normandy. Part 10a deals with this period up to March and April 1945. It covers military events, the underground movement, politics in London, and life in the "Liberated South" after September 1944. Excluded from the first part of the tenth volume are events in the northern part of the Netherlands that remained under German occupation after the autumn of 1944, including the extended and politically complex negotiations with the Germans in the spring of 1945.

In his treatment of the Resistance movement, de Jong demonstrates convincingly the inability of the underground to inflict serious, let alone decisive, damage on the Germans even during the September days in 1944 when the German regime seemed to be on the point of collapse before the apparent surge of the Allied advance through Belgium. This weakness of the Resistance must be attributed in the first instance to the shortage of arms and munitions, but the internal divisions within the Resistance and the mutual suspicion between competing groups also played a part in this failure. Moreover, actual numbers of men available for sabotage or military action remained small until September 1944. De Jong estimates the number of underground workers in the major Resistance organizations before September 1944 to be on the order of twenty-five thousand (p. 80).

The most significant Resistance activities during the period of the Allied advance in the west, apart from assistance to people in hiding, were frequent acts of railroad sabotage. The strike of railroad workers on the orders of the Dutch government-in-exile, issued on September 17, 1944, was successful in that most Dutch railroad workers stayed away from work for the rest of the occupation. De Jong claims, however, that after the initial confusion on the part of the Germans, which created a difficult situation for the Wehrmacht in its defense against the Allied offensive against Arnhem, the Germans managed to meet their own military needs by operating the Dutch net with German personnel.

In his discussion of the much-maligned Dutch Military Government (*Militair Gezag*), de Jong arrives at a moderately favorable judgment. He states emphatically that such an organization was needed to look after the physical needs of the population in the liberated south with the degree of authority that only a military organization that was an integral part of the Allied military structure (SHAEF) could

wield. He admits that the Military Government made a number of mistakes, such as remaining in Brussels during the fall and winter, but he believes that it effectively met the basic needs of the population for food and fuel after the first disastrous months. De Jong provides a detailed if not always crystal-clear picture of the complex competition for authority that went on between the government-in-exile, the Military Government, the Netherlands Forces of the Interior under Prince Bernard, and the "old" local and regional government authorities. His treatment of the abuses of the initial treatment of alleged and real Nazis and other collaborators is characterized by a degree of objectivity that only the passage of time can provide.

De Jong believes that the concept of "renewal" (*vernieuwing*) formed a central theme in the political events of these transitional months. The Resistance and the underground press had called for such a postwar renewal, and Queen Wilhelmina in London had made this theme her own. The protagonists of renewal did not have a very specific picture of the future, but the notion involved in principle a repudiation of the principle of *verzuiling* (the division of society and institutions according to religious lines) and the inclusion of the Resistance in postwar politics and government. The queen's very active role in the London government and her commitment to renewal is shown by de Jong as the main cause of Prime Minister Gerbrandy's decision to reconstitute his cabinet in February 1945. But at the same time de Jong also acknowledges that in the liberated south the tendencies toward new integrated organizations failed when the Catholic hierarchy insisted that Catholics could join only Catholic organizations. Thus even before the end of the war the basic constancy and continuity of political relationships in the Netherlands had reassured themselves.

In this volume de Jong again shows himself to be a lively narrator with an eye for detail and a capacity for balanced judgment. With the publication of the tenth volume, de Jong is coming within reach of the completion of his unique twelve-volume history. The skeptical reader who might wonder where the public can be found for such a massive and detailed history is confronted with the fact that de Jong's work is a best seller in the Netherlands. Nevertheless, the foreign reader will look forward to a condensed version, which may serve a useful purpose to de Jong's compatriots as well.

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ANNA-BRITA LÖVGREN. *Handläggning och inflytande: Beredning, föredragning och kontrasignering under Karl XI:s envælde* [Handling of Matters and Influence: Preparation, Presentation, and Countersign-

ing during the Absolute Monarchy of Charles XI]. Summary in English. (Bibliotheca Historica Lundensis, number 46.) Lund: C. W. K. Gleerup. 1980. Pp. viii, 237. 66 KR.

Anna-Brita Lövgren's study is an exhaustive analysis of the formalities of the decision-making process during the absolutist reign of Charles XI of Sweden. By identifying the officials who actually participated in the drafting of position papers, in their oral presentation to the king, and in the countersigning of the resulting royal letters and decrees, the author believes she can determine just which officials were in a position to influence the decisions of this particular absolute monarch (p. 65).

After discussing the existing state of research and the sources and methods she has employed, Lövgren presents an empirical study and exemplification of the three steps of the decision-making process mentioned above. Her method is a formalistic one, but it is also one that required her to deal in one way or another with over eighteen thousand decisions. Lövgren set herself the task of determining not only who presented the thousands of position papers to the monarch and who countersigned the resulting decisions, but also who drafted those position papers. This last task often required identifying people solely by an analysis of their handwriting.

To achieve an acceptable level of generality, Lövgren chose to study the three elements of the decision-making process by investigating a random sample of decisions reached in a variety of areas. In the section on the drafting of position papers, for example, she chose royal letters and decrees sent to two military commanders (one in the capital, one in the provinces) and to the cathedral chapters of one bishopric in Sweden and one in Finland. Although this method is commendable, it does have the shortcoming of equating decisions on routine matters with decisions of major political or economic import.

The author concludes that, when Charles XI was unable to reach a decision immediately on the basis of precedent or his own knowledge of the case, he referred questions relative to the drafting of position papers either to a chancellery secretary with the requisite expertise or, when such expertise was lacking within the chancellery, to outside experts either in the public or the private sector. Moreover, Lövgren demonstrates that the secretaries countersigning royal documents were seldom involved in drafting and presenting them to the king, and therefore she concludes that the appropriate experts—whether secretaries, royal councilors, or outsiders—had the greatest opportunities for influencing the king's decisions.

Lövgren's results offer no real surprises, especially in view of the fact that—as she herself admits (p. 180)—she has concentrated almost exclusively on *administrative*, rather than *political*, decisions. That the absolute monarch of even a small state such as Sweden—faced as he was with thousands of decisions every year—should rely on available expertise when it came to more or less routine matters of administration is hardly astonishing. Nonetheless, Lövgren has done us a real service by demonstrating in detail the day-to-day decision-making routines followed during Charles XI's reign.

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JENS CHR. MANNICHE. *Den radikale historikertradition: Studier i dansk historievitenskabs forudsætninger og normer* [The Radical Historical Tradition: Studies in Danish Historiography—Its Assumptions and Norms]. (Jysk Selskab for Historie, number 38.) Aarhus, Denmark: Universitetsforlaget. 1981. Pp. 439.

In the 1960s, student criticism of class-biased lectures and research methodology initiated an extended period of self-evaluation among professional Danish historians. Many within the establishment were outraged since the *radikale* historiographical tradition to which they subscribed stressed ideologically neutral, empirically verifiable monographic studies as the ideal application of scientific history. Critics of *radikale* historians pointed instead to the tradition's affiliation with socially conscious but unmistakably bourgeois, aristocratic-radical historians from the turn of the century. The ensuing debate, now well into its second decade, has been lively but often marred by an oversimplistic conception of what constitutes the *radikale* historiographical principles under dispute.

Jens Chr. Manniche's *Den radikale historikertradition* provides the first systematic and intellectually coherent analysis of the tradition that has dominated professional training of Danish historians for nearly a century. Manniche's thesis is that the *radikale* tradition evolved through three distinct phases. After 1870 Kr. Erslev and J. A. Fridericia modified the then dominant historicist tradition by incorporating positivistic principles and methodological criteria modeled on J. S. Mill's *A System of Logic*. Thereafter, Erslev and Fridericia socialized their students in techniques of empirical source criticism and verification, in part to counter what they considered probourgeois, militaristic, and chauvinistic biases in the work of national-liberal historians of the historicist school.

But competing claims to objectivity and empathet-

ic reconstitution of the historical record kept in dialectical tension by Erslev and Fridericia split the second generation of *radikale* historians into two camps. One led by P. Munch and E. Arup stressed the compatibility of objective analysis and an empathetic observer. The other camp, however, with Aa. Friis in the forefront, called for a return to strict Millite principles of objectivity in the evaluation of sources. But both camps were in agreement that the liberal-socialist political sympathies of the first generation should guide all *radikale* historians when they chose topics and problems for research.

The victory of liberal-socialist policies after 1929 inaugurated the third phase of *radikale* historiography. With one's political goals achieved, it became convenient to legitimate and enforce an apolitical, ideologically "neutral" ideal of objectivity. Subsequent *radikale* historians were, therefore, expected to conform to standards of empiricism and objectivity much narrower than those pioneered by Erslev and Fridericia. According to Manniche, Danish historians since the 1930s have gradually truncated *radikale* historiography by eliminating the productive tension between its historicist and positivistic components. Student protests were actually directed against this last phase rather than the tradition as a whole.

Manniche's thesis is well documented, but it is also overburdened by unresolved organizational problems. Copious citations from the writings of *radikale* historians are complemented by insightful synthetic and interpretative comments. It is also refreshing to read a work that places Danish developments within a broad context of West European intellectual history. Nonetheless, Manniche's argument is not always easy to follow. A tendency to tilt with his opponents detracts from the clarity of the exposition, especially in the opening chapters. Consequently, his thesis emerges hesitantly and is often obliterated by picayune theoretical points that should be placed in footnotes. The conclusion should be the introduction and the author's goals limited considerably. Manniche's analysis is more biographical and less sociological than he is willing to acknowledge. On the substantive level, it is puzzling why the author virtually ignores one of the main indigenous origins of the *radikale* tradition—the introduction of Hegelian philosophy into Scandinavia by J. L. Heiberg after 1825.

These weaknesses are outweighed, however, by the book's unquestionable merits. Manniche's sophisticated and nuanced interpretation of *Den radikale historikertradition* should emerge as the Janus of contemporary Danish historiography—a conclusive assessment of past traditions and the foundation for future reassessments.

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R. W. SCRIBNER. *For the Sake of Simple Folk: Popular Propaganda for the German Reformation*. (Cambridge Studies in Oral and Literate Culture, number 2.) New York: Cambridge University Press. 1981. Pp. xi, 299. \$44.50.

The rapid and deep penetration of German society by Luther's Reformation in the 1520s and 1530s is one of the wonders of modern European history. Much has been learned from recent scholarship about the appeal of early Protestantism for the educated classes. Now R. W. Scribner has significantly widened the range and terms of analysis by turning to the reformers' battle for the loyalty of ordinary folk. How was the Protestant message presented to people of marginal literacy or of no literacy at all? How did it find its mark, and what impression did it make? Scribner has assembled a fascinating collection of mostly visual pieces of Lutheran propaganda and analyzed its two most interesting aspects: How did it work? How did those at whom it was aimed receive it? Given the phenomenal success of the Reformation during its initial two or three decades, his answers, and the lessons to be drawn from them, are of interest to all social historians of ideas, not only to those specializing in the sixteenth century.

Beginning shortly after 1520, a huge quantity of hard-hitting propaganda issued from several Lutheran centers (Wittenberg, Nuremberg, Strasbourg) where writers, illustrators, and printers joined to flood the country with the Protestant version of religious and historical truth. As purveyors of information and disinformation, these men shared a number of qualities of mind and temper that help explain their success and the virtual monopoly they achieved over visual propaganda (the very few Catholic examples in Scribner's book are remarkably weak by comparison). They were themselves true believers in the Protestant creed; they showed enormous skill in simplifying this creed and in reifying its abstractions; unlike Lutheran theologians and church officials, they were sympathetic to popular culture, or at least familiar enough with it to use it as the thematic context of their effort to effect a restructuring of common ideas about gospel, law, church, and community. Scribner uses some semiological concepts to probe the relationships between the gamut of images available to contemporary propagandists (paradigms) and the illustrations actually selected by them to gain their objectives (syntagma). He shows that Lutheran artists and caption writers took their signs from the whole range of notions defining the common perception of the world, conceiving it to be their task to rework these familiar pictures and beliefs in people's minds so as to attach them firmly to the Protestant cause. Thus Luther is most often cast in the role of a saint. His enemies take shape in common images of

abhorrence: proverbial home truths about clerics, Antichrist and the events of Doomsday, the topsyturvy world of carnival, scatological humor, demon lore, as well as powerful "root paradigms" such as opposition of light and dark. By choosing signs from these codes and assembling them as "images in syntagma," propagandists produced a visual text that could be read as easily by the unlettered as a printed page was understood by the literate. Most of the messages passed on in this way were negative. Their aim was to involve the follower—emotionally more than intellectually—in a struggle portrayed as cosmic in scale and meaning. A sense of impending crisis was constantly conveyed. Belligerents and articles of belief were marshaled as polar opposites. All contrasts were drawn in black and white; issues and arguments were drastically personalized in a way that became standard practice with cartoonists. Myths were created to replace the old "symbolic universe" with a new integrative reading of current events in the light of history. Beyond explaining past and present, images of visual propaganda were intended to incite to action: this was the major part of the attitude to which propagandists tried to convert their public.

How effective was this effort? Scribner admits that his internal study of the visual evidence cannot give a conclusive answer. To us, as Scribner points out, the enterprise appears to have been powerfully persuasive in creating for the young movement a set of common symbols and an ideology capable of explaining the world and indicating how it must be transformed. But we cannot see these symbols through the eyes of their contemporaries. A more objective assessment must be written from within popular mentality, and this is a difficult task. Behavior is a clue (although its study was not part of Scribner's purpose in this book), but conduct results from so many inner and outer factors that one can never reason backward from comportment to mental states. This leaves us in that ambiguous situation so familiar to historians of popular culture: we know how people were acted upon; we may also know how they themselves acted. But we do not know whether and how the former led to the latter. This quandary acknowledged, it must be said that Scribner's method of analyzing visual propaganda takes us a long step on our way from the outside to the inside of popular mentalities. If we ever get to know them, this stimulating book will be seen to have made an important contribution.

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CLAUS-PETER CLASEN. *Die Augsburger Weber: Leistungen und Krisen des Textilgewerbes um 1600*. (Abhandlungen zur Geschichte der Stadt Augsburg, Schriftenreihe der Stadtarchivs Augsburg, number

27.) Augsburg: Hieronymus Mühlberger. 1981. Pp. 460.

In the late sixteenth century Augsburg was one of the leading textile-producing centers in Europe. Augsburg's fustian cloth was considered among the best produced anywhere, and the city's weavers turned out many other types of cloth as well. By 1600 Augsburg was producing a half-million pieces of cloth annually. Yet most of the people involved in the production of textiles lived on the edges of abject poverty. How this came to be so is fully revealed in Claus-Peter Clasen's wonderfully detailed and meticulous study of the textile industry in Augsburg from the time of the imperially mandated dissolution of the city's guilds in 1548 until the eve of the Thirty Years' War.

Making imaginative use of the available archival evidence, Clasen re-creates the world of those involved in producing textiles—weavers, family members, journeymen, apprentices, servants, suppliers, retailers, and city officials. Regrettably, the sources do not give us much about the individual lives of textile workers, but Clasen has been able to tell us a great deal about their collective lives and their world of work. The narrative is accompanied and illustrated by over one hundred thirty statistical tables. When the minute detail brought forth by the author threatens to overwhelm the reader, he thoughtfully provides concise summaries and a convenient listing of technical terms at the book's end (pp. 437–40).

Clasen reveals to us the number and productive capacities of the weavers, the role of women in the textile industry, the location and quality of the weavers' housing, the types of raw materials they used and where they purchased them, the types of cloth they produced, how the textile industry was regulated, how apprentices and journeymen were trained, and how textiles were produced, tested, and sold. Such a rich profusion of data produces some intriguing insights into the nature of preindustrial capitalism. We learn that even though the textile workers were the most important craft group in Augsburg they were far poorer than many other skilled workers. Indeed, as a group they were kept in a state of financial crisis; many were close to being paupers.

Without their guilds to protect them, Augsburg's weavers were under the thumb of a well-meaning but paternalistic city council. Clasen concludes that a key reason for the poverty of the weavers was that there were simply too many of them for them to receive adequate compensation for their labor. The city's patrician government intentionally restricted production by carefully regulating the number of looms to keep a large number of people employed. In addition, the weavers were limited by a cycle of indebtedness and mortgages, restrictions on raw materials, tough quality standards, and limitations

on trade. Although a safety net of public assistance existed, many textile workers lived a most precarious existence even before the devastation that resulted from the Thirty Years' War.

This is a major contribution to the social and economic history of preindustrial Europe. It will stand as a model of the creative use of archival sources about a major European industry. Historians wishing to generalize about the nature of industry in the early modern European world will do well to consult Clasen's carefully crafted study.

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HANS-JÜRGEN PUHLE and HANS-ULRICH WEHLER, editors. *Preussen im Rückblick*. (Geschichte und Gesellschaft, Sonderhefte, number 6.) Göttingen: Vandenhoeck und Ruprecht. 1980. Pp. 323. DM 36.

RUDOLF VON THADDEN. *Fragen an Preussen: Zur Geschichte eines aufgehobenen Staates*. Munich: C. H. Beck. 1981. Pp. 191. DM 28.

In an article in the *Frankfurter Allgemeine Zeitung* on recent literature about Prussia, Michael Stürmer has suggested that Sebastian Haffner's 1978 work *Preussen ohne Legende* should be considered the most important book on its subject since Otto Hintze's *Die Hohenzollern und ihr Werk*, which appeared in 1915. Since Haffner is an outsider to the profession, this is almost certain to cause objections, but there is much to be said for it. Haffner's was the first book after the abolition of the Prussian state in 1947 by decree of the Allied Control Council to deal with its whole history and to ask basic questions about the reasons for its dramatic rise to eminence and its long death. More than any other single work, it triggered the explosion of new interest in its subject, which has resulted in the so-called "Prussian wave" that has been beating against the walls of publishing houses in both parts of Germany for the past two years.

The books under review here are among the more interesting products of this phenomenon. Rudolf von Thadden's is an attempt to combat what he calls the inaccessibility of Prussian history to the present generation, which has been caused by the burden of the recent past. He admits that Prussia must bear some responsibility for the coming of National Socialism and that "the day of Potsdam . . . is also a date in Prussian history" (p. 22). But he insists that this should not be a barrier to an earnest attempt to penetrate the pre-Hitler past and do justice to Prussia's history in all its complexity, and he undertakes to do this by posing some basic questions (When was Prussia? Where was Prussia? What was Prussia?) and by discussing Prussia's social structure, political character, relationship to Germany, and religious history, with special emphasis on

the way in which they changed from epoch to epoch.

Even the apparently artless questions with which Thadden begins his inquiry prove difficult to answer—to say, as Fontane did, that the real history of Prussia began with the Great Elector is to neglect the vital contributions of Elector Johann Sigismund to the future shape of the realm; to hold that it ended in 1870 is to forget its great services to German democracy in the Weimar period—and his discussion as a whole shows the peril of generalizing about a country in which the element of change was more important than that of persistence. Thadden intimates at one point that the frequent alteration of Prussia's territorial shape, with the concomitant effects of change in available resources and social and national homogeneity, had greater weight than dynastic continuity in determining the nature of Prussia's external policy and the progress of its political institutions. The serious geographical limitations on social integration in the eighteenth and early nineteenth century, for example, explain why the Prussian idea of freedom took a different form from that in the more cohesive national states of the West.

Prussia's role in Germany also changed so often and so radically that the Allied Control Council's argument that it was from earliest times a source of reaction in Germany was demonstrably false. Neither in the reign of the Great Elector, when it attracted refugees from all neighboring states, nor in that of Frederick II, when it was the center and dynamo of the German *Aufklärung*, was it a reactionary state, and before 1933 it was the last bulwark against encroaching Nazism. It is true that on the balance Prussia's conservative social system and backward constitutional structure defeated progressive forces in Germany after 1870 (a development abetted by the state-supporting tendencies of the Prussian church establishment); but even so, "the old Prussia helped the breakthrough of a multitude of modern forces which, without it, would have had far fewer possibilities of development" (p. 100).

More doubtful about this are the contributors to the volume edited by Hans-Jürgen Puhle and Hans-Ulrich Wehler, which is published as a special issue of the journal *Geschichte und Gesellschaft* and has as its basic theme the nature and limitations of the process of modernization in Prussia. The critical note is struck at the outset in Puhle's description of the Prussian *Herrschaftsmonopol* and the way it manipulated modernization so as to deny its logical social and political implications; this is elaborated in chapters on the Prussian military (by Manfred Messerschmidt), the Junkers (by Hanna Schissler), the paternalism of the nobility (by Robert M. Berdahl), and the Evangelical church (by Rudolf von Thadden). Horst Möller warns against too posi-

tive an estimate of the *Aufklärung's* impact on state policy, and Barbara Vogel, in a fine essay on the years 1807–20, shows the role of the bureaucracy in frustrating Hardenberg's political and financial reforms. Notable also in this important volume, for the new light that they throw on the motive factors of economic and social policy, are Jens Flemming's incisive analysis of the slow evolution of legal protection for the agricultural working force and Klaus J. Bade's provocative treatment of the conflict between economic interest and state policy in the question of regulating the inflow of foreign agricultural and industrial workers during the nineteenth century.

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BERND FAULENBACH. *Ideologie des deutschen Weges: Die deutsche Geschichte in der Historiographie zwischen Kaiserreich und Nationalsozialismus*. Munich: C. H. Beck. 1980. Pp. ix, 516. DM 118.

Some years ago Rudolf Vierhaus pointed out the heuristic uses of the notion of a "German way" of special development; now his student Bernd Faulenbach has traced out all the highways and byways of this idea in its career before the Nazi takeover. The message of this rich and persuasive work is not new, but it is delivered with energetic thoroughness and great clarity. Surveying the whole of German academic historiography during the Weimar years (including Meinecke, Hintze, Oncken, Marcks, and scores of others), the author concludes that it amounted to a virtual program of political instruction, which reproached the republic and revered those aspects of the German past least compatible with its survival. Thus one can speak of not merely a German "idea" of history but of a full-blown German *ideology* of history, with particular political, social, and cultural entailments. With rare exceptions, German historical thought performed a sort of *Selbstgleichschaltung* before Hitler, which in alliance with other elements made acceptance of Nazi rule easier. This historically based "ideology of the German way" was not identical with Nazism but was at most points compatible with it; to simplify vastly, Hitler and the historians were fellow travelers—perhaps differing on the final destination or parting along the way but concurring on many of the earlier stops.

The work as a whole shows strong but not intrusive traces of the recent debates concerning the function of history as legitimation or critique. There is no doubt that the author takes his stand with the latter position against the historians he is examining, whose influential works canonized selected parts of the past (Frederick II, Stein, Bismarck), while castigating the "alien" republican order. Faulenbach

candidly avows that "even works on the history of historical writing stand in history, not above it" (preface, p. v), and he then proceeds to show that most of his historian-subjects, for all their theoretical sophistication, forfeited any semblance of critical perspective for the sake of making history an agency of national regeneration. German historiography was locked in a closed, vicious circle of selective perception and self-reinforcing assumptions: "The categorical framework in which German history was comprehended was a product of this history, and to a much greater degree than the historians themselves were aware" (p. 298).

Faulenbach would agree with Georg Iggers, Hans-Heinz Krill, and others that the historicist tradition, with its individualizing and idealizing approach, became ossified and could not provide the means to come to terms with modern society and the modern world. Rather it fostered notions of a unique German destiny and foreordained "protestant" mission, not just for Germany's sake, but for Europe and civilization as a whole. The ideology supporting this mission was a syndrome of elements allegedly ingrained in German history: the primacy of state over society and parties, of foreign policy over domestic politics, of ideas over material culture and economic interests, of "great men" over anonymous structures. Without forcing his thirty-odd historians onto the same Procrustean bed, Faulenbach demonstrates conclusively that they shared a common ideological framework.

This impressive book is filled with perceptive analyses of many individual historians, and its general framework makes it indispensable to anyone concerned with German history and historiography in the twentieth century. The generalist will find clearly defined topics (such as industrialization, Prussia, and 1848) that reward browsing; the specialist will find almost one hundred fifty pages of expansive notes and a very large bibliography. One might have wished for a postscript on the fate of these historians after 1933, but otherwise this volume stands perfectly on its own. After posing the question of continuity in regard to the German past, historians will now have to pose that question in regard to historiography itself.

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KARL-EGON LÖNNE. *Faschismus als Herausforderung: Die Auseinandersetzung der "Roten Fahne" und des "Vorwärts" mit dem italienischen Faschismus, 1920–1933*. (Böhlau Politica, number 3.) Cologne: Böhlau. 1981. Pp. xii, 382. DM 74.

By 1930, the German left had had over a decade to analyze the phenomenon of fascism in Italy and to

learn from the failure of antifascist tactics there. Why then did both the KPD and SPD so seriously misjudge the nature and danger of German fascism, and why were their policies toward Nazism during the final years of the Weimar Republic so ineffective and at times dangerously misguided? The answer, this study by Karl-Egon Lönne suggests, can be found in the way the KPD and SPD press reported on and interpreted Italian developments after 1920. According to Lönne, in coming to grips with the rise of fascism in Italy the *Rote Fahne* and *Vorwärts* developed two antipodal theories of fascism and antifascist defense, interpretations that mirrored and reinforced the revolutionary goals of the KPD and the reformism of the SPD, respectively. Perceptions (or rather, misperceptions) gleaned by the KPD and SPD press from the Italian experience were then applied to Germany, with disastrous effect.

From the start, the *Rote Fahne* portrayed Italian fascism as a creation of the desperate bourgeoisie, which hoped to uphold its imperiled rule by employing fascism to terrorize the proletariat and forestall the impending social revolution. Because fascism was so closely identified with the bourgeoisie, the proletariat's antifascist struggle was seen as one with its revolutionary struggle against the existing bourgeois order. Any forces standing in the way of a proletarian revolution under communist leadership (that is, all noncommunist parties, especially reformist socialists) were in effect aiding fascism. Cooperation with those forces, the KPD press continually reiterated, was tantamount to collaboration with fascism, and was thus out of the question for all genuine (that is, communist) antifascists. The SPD, on the other hand, hoped to stabilize and revitalize the postwar bourgeois system and, through reform, transform it peacefully into a socialist order. The appearance of fascism in Italy did not discredit the bourgeois order for the SPD (as it had for the KPD), for *Vorwärts* interpreted fascism in Italy, and later in Germany, as a movement of only marginal bourgeois groups—of radicalized anticommunist outsiders who intended ultimately to destroy the bourgeois order. For the SPD press, the liberal-democratic *Rechtsstaat* with the values it embodied was the primary bastion against fascism. Since communists, like fascists, were working to overthrow the bourgeois order and the democratic *Rechtsstaat*, they were portrayed by *Vorwärts* as indirectly abetting fascism. The only effective strategy for antifascist resistance, the SPD rank and file were told since 1920, was a socialist-bourgeois coalition committed to upholding the democratic order and isolating the antidemocratic extremists on the right and the left who threatened that order. By 1930, then, coverage of Italian fascism in the *Rote Fahne* and *Vorwärts* had conditioned both communist and socialist readers to

assess fascism in a way that precluded the only realistic and potentially successful antifascist strategy: a united "popular front" of communists, socialists, and the liberal bourgeoisie.

Lönne demonstrates that KPD and SPD policy toward Nazism after 1930 was largely an uncritical application of rigid ideological concepts of fascism that had been formulated in the early 1920s regarding fascism in Italy. Although these findings are unlikely to cause historians fundamentally to revise their views on the German left's antifascist strategy, it is useful to have this link so thoroughly documented. Yet to imply, as Lönne does, that it was the reportage of the *Rote Fahne* and *Vorwärts* that shaped the policies of the KPD and SPD toward Nazism is to overestimate the importance of the press within the respective parties and to put the cart before the horse. Was not the interpretation of Italian, and later of German fascism that surfaced in the *Rote Fahne* and *Vorwärts* more likely the reflection rather than the cause of policy decisions by party leaders?

Because of its organization, the book tends to be repetitive, and Lönne's ponderous syntax more often obscures than clarifies his arguments; sentences like the one on page 340, with its 137 words and 7 clauses, are all too frequent. The publisher is to be congratulated, however, for including an extensive index, something that has been rare in German historical monographs.

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GERHARD HIRSCHFELD and LOTHAR KETTENACKER, editors. *Der "Führerstaat": Mythos und Realität; Studien zur Struktur und Politik des Dritten Reiches*. Foreword by WOLFGANG J. MOMMSEN. Summaries in German and English. (Veröffentlichungen des Deutschen Historischen Instituts, London, number 8.) Stuttgart: Klett-Cotta. 1981. Pp. 465.

The administrative history of the Third Reich is becoming one of the most intensively researched segments of history. This derives not only from the mass fascination with Hitler and the mass documentation left by his defeat but also from the raging debate about the nature of totalitarianism and Hitler's effective power.

A major addition is this collection of papers, edited by Gerhard Hirschfeld and Lothar Kettenacker in German or English, read to the German Historical Institute of London in 1979. These British and German experts have mined well the rich, albeit twisting, veins of documented infighting within the regime. That these specialists

have also honed their skills in repeated action explains why the quality is much higher than in most such collections.

This is true of papers, even those left to the last, that almost fit within the concept of the conference. Alan Milward argues that the accepted thesis of Nazi economic exploitation of Southeast Europe before the war is much overstated. Peter Hüttenberger documents lobbying by pressure groups, notably a bitter struggle between *Dentists* and the more highly trained *Zahnärzte*. Although the scholars fret that their truths could be a trivialization of the regime, which also produced an Auschwitz, most rediscover its omnipresent human pettiness or banality, to use the cliché.

Tim Mason offers the provocative conceptualization of functionalists—who see policy coming out of the structure of the "Führer state"—versus intentionalists—who see policy derived from Hitler's will. Hans Mommsen, a leading "functionalist," argues that the radicalization itself emerged in large measure from the structure's power struggle. The traditional elites, to save themselves, were carried along into dishonor, as when the army let over one million Russian POWs starve to death. "Intentionalist" Klaus Hildebrand, using less provocative concepts of *Monokratie* and *Polykratie*, distinguishes between Martin Broszat's documented chaos and Broszat's more speculative interpretation of foreign policy. Whatever the domestic disorder, it did not halt Hitler's will to conquer.

Jeremy Noakes and Horst Matzerath agree that the mayors were ground down between powerful Gauleiters and the state bureaucracy. Elke Fröhlich, from the Broszat team's researching of Bavaria, describes the shift to peasant support for Hitler to achieve a new farm policy. Horst Gies shows how Darré's farm policy and power was besieged by a host of hostile party organizations.

The volume is a tribute to the dynamism of data gatherers. Their documents have convincingly buried the simplistic version earlier held by experts, and still by the general public, of a monolithic state and party. The volume is less successful in any synthesization of the details for any reader searching for another simple model. The obvious compromise would be to say that policy derived partly from function and partly from Hitler's will, but this would be a nearly useless truism. Yet these essays indicate parameters to the effective "World as Will."

However frustrating any synthesis, this remarkably able research is valuable beyond the confines of Nazi history. It can be a landmark to the enormous complexity of any government or of any human experience. History does not promise a rose garden of obvious truths waiting to be plucked from the tree of knowledge. Unlike that Garden of Eden,

history is filled with thorny facts that may become the historian's unwelcome crown and glory.

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ROBERT ROTH. *Pratiques pénitentiaires et théorie sociale: L'exemple de la prison de Genève, 1825–1862*. Preface by MICHELLE PERROT. (Travaux de Droit, d'Économie, de Sociologie et de Sciences Politiques, number 127.) Geneva: Librairie Droz. 1981. Pp. xiii, 343.

With a few notable exceptions, such as Michael Ignatieff's fine study of the early British penitentiary movement, the most interesting work on the nineteenth-century European prison movement has been done by Michel Foucault or his imitators in France and elsewhere. Although Foucault has been remarkably persuasive in demonstrating the schematic coherence of the political, social, and philanthropic visions of the early reformers, his account of the "birth of the prison" presents some inherent difficulties for historians. First, his conception of historical discontinuity mitigates against historical explanations that stress what reformers may have borrowed from earlier traditions or institutions. Second, Foucault's nearly exclusive emphasis on the ideas of the reformers lessens any consideration of those ideas when they were put into practice. Finally, in his efforts to stress the homogeneity of the new "disciplinary" ideology, he seems to overlook the bitter conflicts that often set the reformers against one another. Robert Roth's fine study of the penitentiary movement in nineteenth-century Geneva attempts to redress these grievances by "relating idea to fact, confronting idea with its realization" (p. 2).

Roth's book is an effort to explore the origins, the life, and the death of the Geneva penitentiary movement through its only significant manifestation: a single modern prison built in 1825 on old ramparts near the outskirts of the town. The Geneva penitentiary was of great interest to contemporary Europeans because it was a partial architectural realization of Jeremy Bentham's panopticon, whose aim was to keep prisoners under constant observation, and because it employed a "mixed" regime halfway between the Auburn system of daily collective labor and the wholly cellular isolation exemplified in the Pennsylvania model.

The international stature of the liberal intellectual elite of Geneva in the immediate post-Napoleonic period was remarkably high. Roth effectively shows how the social idealism of Jean Charles de Sismondi, Pellegrino Rossi, and Étienne Dumont, the translator and editor of Bentham, reached a climax at the

same time as the liberal political allies of this group achieved a solid grip on power in Geneva. He carefully traces the powerful English Quaker influence on the reformers, their firm devotion to utilitarianism, and their fondness in penal questions for abstract and philosophical models. The practically visionary expectations of the Genevan elite in the 1820s about the power of the new penitentiary to reshape the behavior of both inmates and citizens seems to confirm the Foucauldian portrait of these humanist obsessions.

But in the second half of his book Roth reveals how short-lived this enthusiasm for reshaping men and women really was. Although some of the most useful documents of the prison's internal regime have disappeared, Roth manages to show that the utilitarian calculus the first reformers believed would lead to the rapid moralization of convicts was a good deal heavier on the stick than the carrot. In the practical interests of internal discipline the prison administration used the cell, leg irons, and diminished food and salary to punish misbehavior. Even work, so praised by the early reformers for its power to rehabilitate, was apparently regarded by prison officials as a device for ensuring order. Education for the illiterate prisoners was quickly abandoned, as were the public tours that reformers had hoped would display to the citizenry the virtues of the well-regulated prison. By the late 1840s, with optimism flagging, beset by attacks from the suspicious radicals, officials ceased altogether to apply the elaborate evaluative techniques that were meant to distinguish between vicious and virtuous inmates. The patronage societies that had flourished as an extension of the penitentiary beyond its walls also faltered in the 1850s, and the prison, to no one's apparent regret, was dismantled in 1862 for building materials.

Roth might have explained more clearly at the conclusion of his book why the old liberal penitentiary ideals no longer appealed to either the new specialists in carceral matters or the postliberal political leadership. Nonetheless, historians will find this glimpse behind prison walls a useful addition to the rapidly growing literature on institutions of social control.

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RODOLFO SAVELLI. *La repubblica oligarchica: Legislazione, istituzioni e ceti a Genova nel cinquecento*. (Annali della Facoltà di Giurisprudenza dell'Università di Genova, number 49.) Milan: Dott. A. Giuffrè. 1981. Pp. 296. L. 12,500.

In 1575, the new nobles of Genoa insisted on staging a carnival tournament. Innocently festive on

the surface, this act in fact challenged the knightly prerogative of the old nobility to hold tournaments. Members of old noble families condemned the presumption of the new nobles; the new nobles responded by arming themselves, collecting supporters from the lower classes, and provoking street fights with the old nobles. The rival tournaments caused a constitutional crisis in Genoa and, eventually, a civil war.

Strife between the old and new nobles in Genoa was nothing new. The old nobles, with ancient lineages and landed wealth, despised the new men, wealthy from commerce, manufacturing, and banking. In 1528, however, new laws had provided for a sharing of political power, though the old nobles still dominated the government. But distinctions between old and new nobles were maintained and, by 1575, the new nobles were ready to fight for increased political power.

Rodolfo Savelli uses the 1575 uprising as a focus for a wider study of sixteenth-century Genoa. He describes the tensions between nobles, using excerpts from Senate debates, private letters, and anonymous tracts. When civil strife broke out in March 1575, Genoa became an object of international attention, as Spain backed the old nobility and France backed the new. Armed intervention was a very real possibility. Finally, when the old nobles were defeated and withdrew from the city, Savelli describes the efforts of all the powers, and principally the papacy, to mediate the conflict.

It is in the study of diplomatic mediation that Savelli makes his major contribution. Chief among the diplomats was Cardinal Giovanni Morone, the papal legate; by examining his private correspondence, Savelli has reconstructed the slow and difficult process of framing new laws for Genoa. The laws of 1528, suggestions from jurists and private citizens, and constitutions currently in use provided the mediators with a wide range of possible solutions. The process by which they reached agreement on new laws is admirably demonstrated here. The laws of 1576, the product of their labors, endured with only minor changes until the early nineteenth century.

Perhaps it is too much to ask that a constitutional study be interestingly written. Savelli's discussion of the laws of 1528 is, however, unnecessarily confusing. Also, the leaders of the old and new nobles remain shadowy figures, and the fascinating Cardinal Morone is totally undelineated. One wonders, too, why Savelli ignores possible economic reasons for the success of the laws of 1576. These laws eliminated distinctions between old and new nobles and stiffened requirements for nobility by excluding people occupied in certain activities. The nobles after 1576 acted in a united way to govern Genoa. But the stagnation of the Genoese economy after

the sixteenth century might have discouraged potential challengers.

This book, however, is a major contribution to a neglected area of Italian studies. Savelli's command of both printed and manuscript sources is admirable. A useful appendix contains an essay on sources and transcripts of reform proposals.

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ELVIRA CHIOSI. *Andrea Serrao: Apologia e crisi del regalismo nel settecento napoletano*. (Storia e Diritto, number 7.) Naples: Jovene. 1981. Pp. xvi, 477. L. 15,000.

Derek Beales recently stated that Owen Chadwick's *The Popes and European Revolution* (1981) will convince scholars that Catholic countries besides France deserve an important place in "the historiographical map of eighteenth-century Europe" (*Times Literary Supplement*, August 28, 1981, p. 979). If Beales's prediction comes true, Elvira Chiosi's study of Andrea Serrao, a Calabrian prelate, should be eagerly sought by historians of the Enlightenment, because it sheds new light on the religious and intellectual history of Southern Italy during the eighteenth century.

Serrao, the anti-Jesuit and pro-Jacobin bishop of Potenza in the Kingdom of Naples, was the object of a eulogistic bibliography published a few years after his tragic death in 1799—*La Vie d'André Serrao* (1806) by Domenico Forges Davanzati, bishop of Canosa and a friend of Abbé Grégoire. In 1937, Croce called attention to Forges Davanzati's book by sponsoring an Italian translation of it, to which he contributed a preface and annotations. One year later, a 463-page monograph on Serrao and the Jansenists of Southern Italy, carrying the approbation of the Roman Catholic authorities of Palermo and Louvain, appeared in the "Recueil de travaux publiés par les membres des Conférences d'Histoire et de Philologie" of the University of Louvain (G. Cigno, *Giovanni Andrea Serrao e il giansenismo nell'Italia meridionale* [1938]). This has since been considered the standard work on the subject. Chiosi's book convincingly challenges various interpretations proposed in Cigno's monograph, however, and will probably be regarded by many generations to come as a necessary complement to the latter.

It is not an easy task to do full justice to such a detailed and thorough study as this. Suffice it to say that in the first 346 pages Chiosi illustrates Serrao's intellectual development from his education in Rome to his murder by royalist partisans at the end of the eighteenth century. This detailed account of

the prelate's accomplishments is founded on an impressive number of printed and manuscript sources, collected in various libraries and archives in Italy, France, and Spain. The most important manuscripts are included in the appendixes, which form a sizable part of this bulky volume (pp. 347–455). Indexes of the manuscripts, names, and illustrations are also provided.

Chiosi attributes great importance to Serrao's education in Rome, where he was in touch with the Jansenists of the Archetto and studied the works of Gianvincenzo Gravina, a prominent scholar and a staunch adversary of the Jesuits. Cigno could not fully perceive the manifold suggestions that Rome offered to Serrao, because he wrote his monograph before E. Dammig uncovered the complex pattern of Roman Jansenism and before such specialists as C. Ghisalberti and A. Quondam demonstrated the impact of Gravina's thought on Italian culture. Reaping the benefits of this more recent scholarship, Chiosi rightly maintains that the relevance of Serrao's Roman experience has been grossly underrated: "In realtà, quando il giovane calabrese, terminati gli studi, abbandonò Roma, la sua formazione intellettuale, per quanto ancora acerba, era ormai configurata nelle linee essenziali" (pp. 21–22). This is demonstrated through a penetrating examination of Serrao's *De vita et scriptis Jani Vincentii Gravinae* (1758), which was overlooked by previous scholars. Chiosi also insists on the influence of Melchor Cano's *De locis theologicis*, which Serrao praised in his *De Sacris Scripturis* (1763), a work to be regarded as "il frutto più maturo dell'esperienza romana" (p. 45).

Since she attributes such a decisive importance to the Roman period of Serrao's biography, Chiosi tends to limit the consequences of the well-known friendship that existed between the Calabrian clergyman and Antonio Genovesi, a leading figure of the Neapolitan Enlightenment. She criticizes Forges Davanzati and Cigno for having attributed to Serrao the very ideas of Genovesi. True, both of them opposed Scholasticism, the unchecked authority of the popes, the Jesuits, and mortmain, but Serrao held the same ideas before meeting Genovesi. Chiosi finds Cigno at fault on other points as well. For example, contrary to Cigno's opinion Serrao was not a "filippino" (p. 23 n.), and Tanucci did not ask him to write a *ratio studiorum* for the new schools he intended to found (p. 173 n.). However, she still relies on Cigno's authority for many aspects of her research.

It is interesting to note that Serrao created a considerable fortune for his family, as attested by a *fideicommissum* (1793) published by Chiosi in appendix 8. He also founded in Calabria, over the ruins of Castelmonardo, destroyed by the earthquake of 1783, a new city called Philadelphia after the Ameri-

can one created by William Penn. The coat of arms of this new city was Masonic, which proves that Serrao was himself a Freemason, a fact that Cigno refused to admit.

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FRANCESCO BALLETTA. *Il Banco di Napoli in Calabria al tempo della prima guerra mondiale*. (Institut International d'Histoire de la Banque; Gens d'Affaires, Banques, Monnaies, Finances, number 5.) Geneva: Librairie Droz. 1979. Pp. 239.

Although students of Italian history have long recognized the enormous economic impact of the First World War, scholarly attention has focused primarily on the dramatic expansion of those industries in the North that directly produced war material. With an eye to postwar developments, historians have stressed the growth of new financial-industrial empires in key sectors and the emergence of a "military-bureaucratic-industrial" complex in Rome. In their treatment of the perennially depressed economy of the South, they have emphasized the exclusively regressive effects of the war, citing above all the impoverishment of southern agriculture as a result of inflation, labor shortages, commercial disruption, and government policies.

This study of the Bank of Naples's operations in Calabria offers a more nuanced and less bleak portrait of the war's economic repercussions in one of Italy's poorest regions. Francesco Balletta shows how the war partially compensated for the damage to agriculture in Calabria by stimulating a major influx of money into the region. Such wealth enabled leaseholders to invest in land and allowed the families of agricultural laborers to improve noticeably their standard of living.

Both in its time frame and content, Balletta's study is broader than its title might suggest. A long introductory chapter combines a lucid summary of socioeconomic conditions in prewar Calabria with an overview of Italy's banking system and wartime economy. The real importance of the study lies, however, in Balletta's analysis of the documents in the *Archivio Storico* of the Bank of Naples. During the war years, the Bank's receiving operations in Calabria did not just expand, but did so at a much faster rate than its branches in other regions. Interest-bearing accounts and bonds of various sorts all displayed marked increases. Balletta traces this expansion to sales of farm products, gains made by leaseholders from frozen rents, the higher incomes of worker families, and state subsidies to the families of soldiers and war dead. Conversely the Bank's lending operations tended to contract, though in

certain cases the reduced demand for loans actually reflected the improved economic situation of the less affluent classes.

The chief shortcomings of the study are more in the area of presentation than interpretation. By devoting separate chapters to each province, Balletta gives his work a quality of repetitiveness and excessive overlap. In addition to some 89 charts and graphs, there is far too much specific statistical data in the body of the text, so that the broader trends and tendencies often become lost. Finally, Balletta might have explored further the larger social implications of his findings, especially for postwar events in the South. But these limitations do not detract seriously from what is a useful and solidly researched monograph on a neglected aspect of Italy's wartime economy.

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LISANNE RADICE. *Prelude to Appeasement: East Central European Diplomacy in the Early 1930's*. (East European Monographs, number 80.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1981. Pp. viii, 218. \$17.50.

This book deals with a fateful episode in interwar European diplomacy: the last and abortive attempt at establishing a comprehensive system of security in the Continent's troubled eastern part. The idea of such a system was born out of fears for the regional status quo in the wake of the Nazi revolution in Germany. Of all the European reactions to this event, the most significant were the moves of the French, whose foreign policy was in the hands of Louis Barthou. His first step was aimed at achieving a Franco-Soviet rapprochement, and this in turn led to proposals for a multilateral pact that was to include most of the Eastern European countries as well as Germany and France. Barthou made this effort, aimed at containing German revisionism peacefully, into the official French policy, and he pursued it tirelessly in arcane negotiations. His death by assassination in October 1934 slowed progress for the eastern pact. Barthou's successor, Pierre Laval, was a proponent of Franco-German friendship. He was ready to jettison the proposals for the eastern pact, although, under pressure from the public and his colleagues in the government, he carried on with Barthou's projects. It was Laval who signed the Franco-Soviet Mutual Assistance Agreement in 1935. By this time the original protagonists of the eastern pact were showing a lack of interest in its future. The Russians appeared satisfied by their agreement with France, while the French saw further negotiations on the pact as a means toward

effecting an understanding with Germany. The policy of containment began to give way to illusions of appeasement.

Lisanne Radice's book devotes special attention to the diplomacy of Poland, which is termed here as the lynchpin of the eastern pact security system. Poland's overriding concern was to maintain the equilibrium between Germany and the Soviet Union, a policy based on nonaggression treaties with both neighbors. By contrast, the alliance with France had for a long time been losing its value for the Poles, who ceased to believe that France could be moved for the defense of their country. Without rejecting the proposals for the eastern pact outright, the Polish foreign minister, Jozef Beck, kept raising obstacles toward its acceptance. The main, though seldom mentioned, Polish reservation was the prospect that in the event of war Poland would become a battleground and would be occupied by foreign armies unlikely to leave Polish territory. In Radice's view it was thus the Polish government that saw the eastern pact with most realism. All the same, she perceives the Polish noncooperation, along with German intransigence and the change in the French foreign policy, as one of the three major factors in the failure of the pact.

This is a very detailed study of one fragment of pre-1939 diplomatic history. Dispatches, notes, conversations, and interviews come under the magnifying glass of minute analysis, and the reader has to be alert in following all the threads of the complex pattern of the negotiations. The focus of the narrative shifts relentlessly from Paris to London, Moscow, Warsaw, Berlin, Prague, and Rome. This method better explains the story of a particular move or countermove on the diplomatic chessboard than the inner dynamics of a given country's foreign policy. By way of stylistic contrast, an extensive conclusion in the form of an interpretative essay offers an incisive overview of European diplomacy in the years between Hitler's rise to power and his first aggression in the Rhineland.

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YITZHAK ARAD. *Ghetto in Flames: The Struggle and Destruction of the Jews in Vilna in the Holocaust*. New York: KTAV. 1980. Pp. 500. \$17.50.

The city of Vilna, the "Jerusalem of Lithuania," played a central role for many centuries not only in the cultural life of Lithuanian Jewry but also in that of world Jewry. Although the economic well-being of the Jewish community declined precipitously after World War I, when the city came under Polish administration, its prominent role as a center of

Jewish learning was maintained during the interwar period through a vast network of educational and cultural establishments. Tragically, this community was among the first to be destroyed when the Nazis began the implementation of their program for the Final Solution of the Jewish question in Europe. Prior to the German onslaught, the community was subjected to mounting antisemitism and discriminatory practices on the part of the various masters that ruled the city. Shortly after the Poles were defeated in the Blitzkrieg of September 1939, the city came for about seven months under a Lithuanian administration that was marked by a series of anti-Jewish riots. Between June 15, 1940, and June 23, 1941, when the city was under Soviet rule, all Jewish organizations and institutions were disbanded and more than five thousand Jews identified as "anti-Soviet elements" were deported to Western Siberia and other parts of the USSR. When the Germans occupied Vilna on June 24, 1941, the Jewish community numbered approximately sixty thousand, including a few thousand refugees who had come into the city from German-occupied Poland.

Yitzhak Arad's study is an in-depth, fully documented account of the life, struggles, and systematic destruction of this Jewish community during the three years of German occupation. (The Red Army liberated the city on July 13, 1944.) It describes in an unemotional yet suspenseful style the Jewish community's valiant though basically Sisyphean efforts to survive in the face of hunger, disease, and the recurring operations (*Aktionen*) by the SS bent on its total destruction. It is history at its best, for it describes and analyzes this doomed struggle in the context of its time and on the basis of contemporary documents emanating from many sources, including the Nazi bureaucracy as well as the ghetto administrations, the resistance organizations, and the victims themselves. Divided into twenty-six chapters grouped under five headings, the study begins with a succinct but thorough overview of the Jewish community just prior to the German invasion. This is followed by a detailed account of the Final Solution process, including the expropriation, ghettoization, and extermination of the Jews. The latter was carried out in waves by special *Einsatzgruppen* units at Ponar, a wooded area about eight miles south of the city. The author describes with great care and objectivity the inner workings of the ghettos, the relationships between the Jewish Councils (*Judenräte*) and the ghetto police, the ghetto population, and the resistance movements, as well as the collective efforts of the Jewish leaders to save whatever could be saved in the uneven struggle with an enemy who could always determine the time, place, and method of extermination. The helplessness and defenselessness of the Jews in a completely hostile environment, their limited and usually

ephemeral options in the doomed struggle for survival, and the agonizing decisions required of their leaders in response to the ever-recurring cruel and inhuman orders of the Nazis come into sharp focus in Arad's dispassionate narrative. Particularly poignant is the portrait of Jacob Gens, who first served as the head of the ghetto police and later became, in effect, "the absolute ruler of the ghetto" (July 12, 1942–September 14, 1943). The author reviews, with understanding and compassion, the circumstances that led this former captain in the Lithuanian army to turn over the weak and the old to the executioners in an attempt to "save the strong and the young." It was the same objective—to save whatever could be saved—that drove him and his supporters to surrender Yitzhak Wittenberg, a leader of the underground resistance movement, to the SS and to emphasize the "work to live" program as a means of survival for the ever-diminishing remnant. In retrospect we know that none of these strategies could have succeeded, for the Germans were intent on the total extermination of the Jews. In the sinister designs of the Nazis these Jewish leaders ended up, as Arad notes, unwitting tools in the implementation of a program of whose scope and dimensions they themselves were not originally aware. This book is a valuable contribution not only to the literature pertaining to the Holocaust in Lithuania but also to that of Europe as a whole, for the fate that befell the Jews of Vilna was shared by most Jewish communities in Nazi-occupied Europe.

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ŠIME PERIČIĆ. *Dalmacija uoči pada Mletačke Republike* [Dalmatia on the Eve of the Fall of the Venetian Republic]. Summary in Italian. (Centar za Povijest Znanosti, Odjel za Hrvatsku Povijest, Monografije, number 10.) Zagreb: Sveučilište u Zagrebu. 1980. Pp. 266.

From 1409 to 1797 Dalmatia was, in effect, a Venetian colony supplying the Italian city state with olives, fish, and soldiers—especially soldiers. According to Šime Peričić, the Venetians obtained more than twice as many soldiers from Dalmatia as they got from "the Venetian *terraferma* and the Levant combined" (p. 234). Equally important, Dalmatia served Venice as a Catholic bulwark against further Turkish expansion into the West. Its ratio of eleven priests for every one thousand believers was "the highest in Europe" (p. 55).

Peričić's book is a narrow study of Venetian rule in Dalmatia during the twenty-seven years preceding the fall of the Venetian Republic in 1797. The author depicts Dalmatia as an economic and cultural

backwater that stagnated for centuries under Venetian rule. In this semifeudal environment, agriculture often failed to supply even the needs of the local population. Peasants were burdened with heavy obligations and corruption was omnipresent. The land was broken into such small parcels that efficient cultivation was impossible. The methods of agriculture in 1770 were little different from what they had been centuries earlier. The inertia of the local population and the perpetual Turkish threat made matters worse.

Perićić insists that the stagnation of Dalmatia and the decadence of Venice were the result of a Venetian failure to comprehend the implications of such momentous events as the Industrial Revolution in England and the bourgeois revolution in France. This may be, but he clearly goes too far when he blames Venice for clinging to an obsolete mercantilism. Western Europeans living in 1770 would have been surprised indeed to learn that mercantilism was obsolete: *The Wealth of Nations* did not even appear in print until 1776. In fact, the basic thrust of this work leads in an opposite direction.

Perićić shows that by the middle of the eighteenth century the Venetians had come to see the need for making economic reforms in Dalmatia, and his detailed monograph demonstrates that toward the end of the century the steps they took produced something of a minirenaissance. Schools were established, books were published, theaters were opened, swamp lands were drained, large aggro-estates were created, the shipbuilding industry was significantly expanded, and crafts essential to the Venetian economy were encouraged. As the author shows, during the closing decades of the eighteenth century, Venice granted Dalmatia more freedom of trade. Internal and external trade grew, warehouses were erected, trade fairs were organized, and credit facilities were established.

Perićić argues that all this failed to transform the Dalmatian economy, and he blames custom barriers, the diversity of currency and measures, and the instability of prices. He is certainly correct in noting that Venetian reforms failed to create the millennium and that reform was only partial. Still, his own study suggests that a significant beginning had been made. Given the fact of the fall of the Republic of Venice in 1797, it is impossible to assess fairly the potential long-term effectiveness of the Venetian reform program.

This is the first full-scale study of Dalmatia in the late eighteenth century and, despite its somewhat heavy-handed style, it is a significant contribution to our understanding of both Dalmatia and the Venetian Republic in the Age of Enlightenment.

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ROBERT J. DONIA. *Islam under the Double Eagle: The Muslims of Bosnia and Hercegovina, 1878–1914*. (East European Monographs, number 78.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1981. Pp. xix, 237. \$17.50.

The Austro-Hungarian occupation of Bosnia-Hercegovina from 1878 to 1918 has never been adequately investigated, and, therefore, this study is especially welcome. Robert J. Donia briefly discusses the Austro-Hungarian administration in Bosnia-Hercegovina and notes that Austria-Hungary retained the Ottoman feudal land tenure system. Some measures were introduced to improve relations between peasants and landlords, but neither the peasants nor the Muslim landlords were content, the former because they did not gain enough, and the later because they had lost too much.

Fully cognizant of the close ties between nationalism and religion, Austria-Hungary sought to establish control over the religious communities in Bosnia-Hercegovina. Benjamin von Kállay, the joint finance minister from 1882 to 1903, was determined to establish imperial sovereignty "over all Bosnian religious institutions," as well as to avoid anything that could lead "to the creation of a Mohammedan nation" in Bosnia (p. 161). In 1882 he undertook to regulate Muslim affairs through the establishment of the office of *reisül-ulema* (religious head), who was nominated by and responsible to the emperor, and the creation of the Provincial *Vakıf* Commission in 1883. The Muslims protested against the loss of their various prerogatives and control over their religious and cultural life. Of special import is the author's assessment of various types of Muslim opposition—armed resistance (1878, 1882), the founding of reading societies (*Kıraethane*) from 1888 on, and petitions and protests. Apart from these forms of resistance, many Bosnian Muslims opted for emigration to Turkey, from where they continued to speak out against the Austro-Hungarian occupation policies.

In the 1890s one witnessed religious violence and the emergence of Bosnian Muslim political activism. The conversion of Muslims to Catholicism frequently triggered public disorders and culminated in the issuance of a law on conversions (1891). The Bosnian Muslims—landlords, merchants, craftsmen, peasants, and the *ulema*—disagreed among themselves on many points, but they rallied in defense of Islam. In the forefront of the protest movement were the Muslim communities of Sarajevo, Mostar, and Travnik, each playing a particular role in it. By examining archival materials and other sources, the author systematically follows the development of Muslim political action and analyzes the contents of

petitions sent to the emperor and government authorities, revealing the social background as well as religious and political views of the petitioners. Apart from questions of local concern, the Muslims of Bosnia debated the same philosophical and political issues (compromisers and modernists versus traditionalists and zealots) as did Muslims the world over. In 1899 the Bosnian Muslims launched a movement for religious and cultural (*vakıf-mearif*) autonomy, and after much wrangling among themselves and intermittent discussions with the Austro-Hungarian authorities, in December 1906 they proclaimed the creation of a political party known as the Muslim National (or Popular) Organization. The party became the official spokesman for Muslims and published its own journal, the weekly *Musavat* (*Equality* [1906–11]).

For a moment the annexation crisis of 1908–09 aggravated relations between the Muslims and the government. After passions subsided, the government accepted the Muslim demand and on May 1, 1909, approved the statute for Muslim religious and cultural autonomy. With this the Muslims attained their main objectives in regard to religious and cultural autonomy but won only symbolic gains in “the field of property law” (p. 172–73). Many earlier institutions (*reisül-ulema*, *ulema meclisi*) were retained, but the way they were elected was modified. The ties between the Bosnian Muslims and the *şeyhülislâm* (supreme religious head in Istanbul) were restricted, and those with the emperor were defined in a manner acceptable to the Muslims.

The new relationship between the Muslims and the government along with the special circumstances that emerged from the Balkan Wars (1912–13) and World War I brought the Muslims into ever closer dependence on Austria-Hungary. The author’s findings have led him to conclude that the Muslim activists were “authentically autonomous political actors seeking advantages for their own group” (p. 186) and wished to have their own religious administration. Regardless of whether one shares this conclusion, the author has given us an excellent study of a highly complicated subject. He has also demonstrated intimate knowledge of the history of Bosnia-Herzegovina and the ability to handle sensitive ethnic and religious issues with objectivity and understanding.

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VALENTIN AL. GEORGESCU and PETRE STRIHAN. *Judecata domnească în Țara Românească și Moldova, 1611–1831*. Part 1, *Organizarea judecătorească*, volume 2, 1740–1831 [Princely Justice in Wallachia and Moldavia, 1611–1831. Pt. 1, Judicial Organiza-

tion vol. 2, 1740–1831]. Summary in French. (Biblioteca Istorică, number 61.) Bucharest: Editura Academiei Republicii Socialiste România. 1981. Pp. 232. 16 L.

Valentin Al. Georgescu and Petre Strihan have been known for a long time as outstanding scholars of the post-Byzantine legal history in Southeast Europe, particularly in the principalities of Moldavia and Wallachia. The study under consideration here represents an almost definitive work on a much-debated and controversial problem: the character of the judicial institutions in the Romanian principalities at the time of their early modernization, that is, from Prince Constantin Mavrocordat’s reforms of 1740 to the introduction in 1831–32 of the Organic Regulations, the country’s first modern constitution.

This is an outstanding study based mostly on primary sources and displaying an impressive erudition: it goes into great detail regarding the judicial reforms of several Phanariot princes as well as into the analysis of the different institutions sharing judicial power, from the prince to several departments and to the great boyars having the right to judge. Interesting remarks are made about the country’s estates as well as about the hundreds of memoirs and projects presented by the boyars in order to reform the administrative structure of the principalities along the principles of the Central and East European Enlightenment and enlightened despotism.

Several of the conclusions the authors have reached are rather striking and convincing: the study proves that, despite the general political decadence brought upon the principalities by the Phanariots, the legislative autonomy of the country had never been infringed upon by the Ottomans. New also is the conclusion that, despite the many and well-known Phanariot abuses, from a strictly institutional point of view the princely power experienced a period of decadence, checked and limited by both the strong social and political elites as well as by a complex law system. Coming from a historiography that has been so uselessly politicized in the past few years, Georgescu’s and Strihan’s book provides the Western scholar with a striking and refreshing lecture.

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VIRGIL CÂNDEA. *Rățiunea dominantă: Contribuții la istoria umanismului românesc* [Reason Prevailing: Contributions to the History of Romanian Humanism]. Cluj-Napoca: Dacia. 1979. Pp. 381. 25 L.

Virgil Căndea, perhaps the most brilliant cultural and intellectual historian of Romania, has written an

intelligent and original book on the humanist tradition in Romania. The volume consists of several essays and studies, all designed to place Romanian humanism in the framework of Romanian history and of European humanism in general. The most interesting essays are devoted to the history of Romanian humanism and to the character and role of the intellectual in Southeastern Europe in the seventeenth century. The essay on the historiography of Romanian humanism is rather technical, and the two studies on the works of Udriște Năsturel and Nicolae Milescu are of interest primarily to specialists of premodern Romanian history and to linguists and philologists. All contributions, however, reflect meticulous scholarship and original thought.

The underlying thesis is that from as early as the sixteenth century certain Romanian intellectuals sought to emulate and understand the lessons and ideas that affected European thought and historic evolution since the Renaissance, all with a view of defining and delineating the character and aims of a nascent national culture. Căndea recognizes that the efforts and cultural activities of Romanian humanists were both imperfect and of lesser significance than that of their successors in the Age of Enlightenment, but he rightly underlines the importance of their work and their impact on political theory and even practice in the seventeenth and later centuries.

The relationship between Romanian humanist thought and the evolution of a Romanian national consciousness is stressed but not exaggerated. The general "Levantine" quality of Romanian intellectual life in the seventeenth century is justly emphasized as is the "cosmopolitanism" of the interests and work of Nicolae Milescu, whose *Chinese Travel Journal and Description of China* remain among the most important works in Romanian cultural history. Căndea's book is both sophisticated and informative. It augurs well for Romanian historical scholarship.

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THEODORE PROCHÁZKA, SR. *The Second Republic: The Disintegration of Post-Munich Czechoslovakia, October 1938–March 1939.* (East European Monographs, number 90.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1981. Pp. vii, 231. \$20.00.

After Munich Czechoslovakia found itself in the position of a tragic hero in a surrealist, Kafkaesque drama. The lonely and frustrated struggle of its people with the incomprehensible forces shaping

their destiny was doomed to failure. Surrounded by hostile nations and abandoned by their allies to the arbitrary decisions of Nazi Germany, they lost all hope and gave in to defeat.

This book deals with the disheartening period in Czech history from the Munich Agreement of 1938 to the establishment of a German protectorate over Bohemia and Moravia in March 1939. It is divided into five chapters, which cover in detail the implementation of the Munich Four Powers *Diktat* and the negotiations carried on by Prague with Berlin, Bratislava, Budapest, and Warsaw. This work, which originated in 1954 as a dissertation at the Sorbonne, is based on published documents and contains some interesting information received from personal interviews with leading Czech politicians. The material is further updated with more recent sources and documents from the National Archives in Washington, but no Czech archival material was used. This, of course, weakens the value of this work; and, even after major rewriting, the book still retains the flavor of a dissertation: it is strong on facts, but weak on analysis. On the other hand, it is precisely the detailed presentation of the day-to-day events, which makes the book so informative and valuable.

Here and there, we find some outdated interpretations, as on page 45 where the author implies that one of the reasons for Warsaw's policies toward Prague was the Polish desire to be treated as a great power. Apparently, old myths die hard. The Poles never claimed that Poland was a great power, nor did they demand that it be treated as such. Rather, their policies were often motivated by a number of assumptions and principles that were to guide their actions. One of them was the old, *nic o nas bez nas* (nothing about us, without us). On that basis they expected to influence international decisions that affected their own national interests. But such attitudes on the part of a lesser power are often perceived by greater powers to be presumptuous; and here, perhaps, we might find the major source of this myth. In another example—in chapter 5, which deals with the Western response to the German occupation of Prague—the author does not point out that in historical perspective the occupation played a crucial role in the re-evaluation of English and French policies toward Nazi Germany that led to the change in their appeasement policies.

The above critical remarks do not detract from the general value of this book, which should find its place on East European shelves of every college library. Both students and specialists will find the book fascinating reading and full of useful and detailed information. The bibliography is comprehensive and generally up to date. Furthermore, the publishers should be commended for bringing out

into print valuable works like this, even though they are based on older dissertations.

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EDWARD TABORSKY. *President Edvard Beneš: Between East and West, 1938–1948*. (Hoover Press Publication, number 246.) Stanford: Hoover Institution Press. 1981. Pp. xi, 299. \$19.95.

The longtime foreign minister and second president of the Czechoslovak Republic, Edvard Beneš, has not been well served biographically. Unlucky in his critics, he has been equally unlucky in his hagiographic biographers and supporters. At last, Beneš has found his champion in Edward Taborsky, his former personal secretary and legal adviser. In this most lucid and penetrating treatment of Beneš that has yet appeared, the author contributes significantly to our understanding of Beneš, the noble and sympathetic loser who dominated the Czechoslovak political scene between 1935 and 1948 and enjoyed an international prestige considerably out of proportion to the size of his country.

An able negotiator with a sense of purpose, Beneš fell victim first to Hitler and the Western policy of appeasement and then to Stalin and the failure of the West to support democracy in Central Europe. Never starry-eyed or romantic, Beneš fervently promoted the idea of peaceful East-West cooperation as the guarantee of Czechoslovak independence. Alas, his belief in Soviet good intentions and Stalin's trustworthiness proved to be a deep disappointment. Six months after the Communist takeover in 1948, Beneš died a broken man, confessing to his associates that "trusting Stalin and Gottwald was his 'greatest mistake'" (p. 294).

Taborsky's approach is essentially topical and analytical. The subjects covered range from the Munich crisis and the dismemberment of the republic to the outbreak of the Cold War, the Communist coup, and Beneš's resignation and death in the summer of 1948. The major theme of Taborsky's analysis is his attempt to answer the question, How much of the blame for the president's failure in his struggle to preserve democracy and national independence can be laid at his door. Exploiting his own wartime diaries, reminiscences, and private archives, and relying heavily on oral interviews and other materials, Taborsky convincingly refutes most of what was said by Beneš's opponents. Analyses of Beneš's relations with the Western powers and the Soviet Union, and of his anxiety about the Communist menace to his country fuse into a coherent image of the president's personality. What emerges is a balanced portrayal of a moderate statesman and

experienced diplomat whose tragic end illustrates the fate of a small Central European country situated tragically at the crossroads between East and West. Taborsky's summing up is thoughtful. While arguing that Beneš's policies failed largely because of factors beyond his control, the author wisely does not overlook Beneš's shortcomings, the gravest among them being the lack of providing "the forceful leadership that was so desperately needed" (p. 229) and the tendency to avoid "calling a spade a spade" and instead to search for compromises and vague formulas accommodating everyone (p. 239).

The compact volume makes no effort to cover any aspect of Beneš's political career in detail and neglects domestic politics. This closely argued, interpretative study is a book that readers will find instructive and a valuable point of departure into the present troubled decade. Its subject calls out for further reflection if we are to get the East-West confrontation correctly in focus.

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MARTIN R. MYANT. *Socialism and Democracy in Czechoslovakia, 1945–1948*. (Soviet and East European Studies.) New York: Cambridge University Press. 1981. Pp. ix, 302. \$42.50.

This doctoral dissertation ripened into a handsome, flawlessly produced book with copious notes, abundant bibliography, and diacritic marks in the right places. The topic, though limited to a short period in the life of one small country, is of great importance. As the author asserts, Czechoslovakia "remains a unique example of a democratically elected multiparty government implementing socialist changes in what, even then, was one of the most advanced countries in Europe" (p. i). Yes, indeed, nowhere else but in Czechoslovakia did the Communists win in a free election (1946), an achievement the population has not appreciated being reminded of in the subsequent decades.

The author, Martin R. Myant, sets himself the task of explaining how this type of socialism was accomplished and why it took the form of an unsuitable Soviet model. He disassociates himself emphatically from émigré writers who allegedly reduce the Czechoslovak drama to a simpleton's formula of a perfidious Communist plot. Instead, the author promises to find the answer in the roots of the Cold War.

The book contains several sets of useful data with regard to the political composition of the trade union membership, the national committees, the socioeconomic profile of the political parties, and so forth. Mention is made of little-known incidents of

postwar antisemitism in Slovakia (p. 102) and of the thoroughly unknown food aid from the Red Army (p. 56).

Myant's supervisor of his doctoral thesis at Glasgow University was Vladimír Kusín, a fine scholar of impeccable credentials. Thereafter, the author spent a year in Prague in different company. His supervisors, who according to the acknowledgment gave helpful advice, were Čestmír Amort, whose falsification of archives resulted in one of the best-known scandals in preinvasion Czechoslovakia, and Václav Král, the notorious Stalinist, who is generally regarded as the Pol Pot of Czechoslovak historiography. Král's writings in which he labels Gustáv Husák a bourgeois nationalist vermin are, however, omitted in Myant's bibliography.

Myant's access to Prague was not toll free. Contrary to the title of the book, the focus is on the Communist party, which he treats with considerable respect. Its competitors are treated with condescending disdain. The author's handling of his sources is highly selective, and some of his reasoning brings back the memory of the revisionist historians of the simpler variety. The author castigates the United States for the failure to provide economic assistance, yet acknowledges that Czechoslovakia accepted the American invitation to attend the Marshall Plan conference in Paris and was forced to withdraw upon Soviet insistence. The Cold War is blamed on the Truman Doctrine of 1947 that presumably moved Stalin in 1945 to concentrate "on strengthening relations with Eastern European states" (p. 166)—an extraordinary way to characterize the sovietization process in the area.

The author presents a scenario according to which the Soviet Union has played a minimal role in the formation of Czechoslovakia's future. The role of V. A. Zorin in Prague in the February 1948 crisis is touched on and its importance is dismissed (p. 210) in disregard of all the evidence to the contrary. The author also disregards all the evidence that came to light during the 1968 investigations that Jan Masaryk was murdered by Soviet agents. The number of extraordinary observations grows in the concluding parts of the book. For example, Myant asserts that the Social Democrats dreaded a Communist defeat just as much as a Communist victory (p. 215). "It is not even obvious what émigrés hoped to achieve in the West," he says (p. 217). Perhaps they preferred avoiding imprisonment, which was the fate of many. Some were even executed. Even the "normalized" archives in Prague at M. V. Myant's disposal cannot entirely unmake this segment of history. The book is thus seriously flawed.

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STEFAN KIENIEWICZ. *Pomiędzy Stadionem a Goslarem: Sprawa włościańska w Galicji w 1848 r.* [Between Stadion and Goslar: The Peasant Question in Galicia in 1848]. Wrocław: Ossolineum. 1980. Pp. 155. 30 Zł.

The subtitle of this essay by Stefan Kieniewicz summarizes its topic and concentration. It is a complement to his monograph, *Ruch Chłopski w Galicji w 1846* (The Peasant Movement in Galicia in 1846) published in 1951, and to his article, "Sprawa włościańska w Galicji w 1848" (The Peasant Question in Galicia in 1848) published in 1950, not only chronologically but also in the area of the issues and sources considered.

Count Francis Stadion, whose name is in the title, was the Austrian governor of Galicia and from 1849 was the minister of interior in Vienna. He represented one road in the emancipation of peasants. It also was a road toward winning for Vienna the masses that were not so clearly conscious of their Polish nationality and overwhelmingly trusted the emperor while defying his bureaucracy and gentry. (Even some forty years later, the peasant leader Wincenty Witos noticed that many peasants of that region had no clear notion of their Polish nationality and that many of them considered themselves emperor's people.) Julian Goslar, the other name in the title, was born into a peasant family, and he was fully immersed in the rural environment. He represented an opposite road for the social and national emancipation of peasants, a Polish national revolutionary and democratic road. This road was inspired and supported by the Polish Democratic Society in France that acted in Galicia through the local organization "Stowarzyszenie Ludu Polskiego" (Society of the Polish People) and through emissaries coming from France. After being imprisoned for one year in the Kufstein fortress (Tyrol), Goslar again became involved in revolutionary activities. He was rearrested, sentenced to death, and executed in 1852, at the age of 32.

The social and economic sides of the emancipation of peasants involved political and broad, national issues, Polish and Ukrainian, as well as Austrian. Any resolution of the peasant emancipation problem unavoidably led to the improvement or deterioration in the national struggle. Thereafter, a competition for winning peasants developed. The polarization of the Polish gentry resulted from motives rooted in their selfish economic interests based on the fear of peasant revolution, so fresh in the recollection of the 1846 slaughtering of the gentry on one side, and in a deep concern for winning peasants for the national cause on another side.

This study deepens and broadens knowledge of the events of the "Spring of Peoples" in Austria,

particularly in its Polish lands, and of the assembly that met in Vienna and that moved to Krems in October 1848 beyond the standard information usually used in university textbooks. The author, with mastery and on a broad source basis, assesses events that involved the Austrian administration and reached all social groups in Galicia. He has produced a multilayered picture of the society in the south of the partitioned Poland that was torn by that time between the *österreichische Staatsnationalität* and Polish national aspirations that needed peasants to be successful.

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JOAN S. SKURNOWICZ. *Romantic Nationalism and Liberalism: Joachim Lelewel and the Polish National Idea*. (East European Monographs, number 83.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1981. Pp. 202. \$16.00.

In the preface to her book, Joan S. Skurnowicz defines her aim as the introduction of "Lelewel's works to a wider audience [that is, in the West] . . . by discussing his contributions to the development of Polish historiography, his contributions to the writing of Polish history, and his tireless efforts on behalf of Poland" (p. 3). Despite the expectations raised by the title, what in fact she has given us is a straightforward biography of the historian from the cradle to the grave. A penultimate chapter summarizes, in chronological order, his major works on Polish history, and the final chapter, though entitled "General Observations and Conclusions," merely recapitulates what has gone before.

Unfortunately, Skurnowicz makes little attempt either to relate Lelewel's ideas on the nation to the concept of Polish nationalism as it evolved during the first half of the nineteenth century or to place his "political republicanism and social democracy" (p. 103) within the framework of contemporary European liberal and radical thought. Lelewel spent some of the most creative years of his life in Wilno (Vilnius), a city located in the Polish-Lithuanian borderlands, but the author does not seek to explain the contradiction between Lelewel's advocacy of the pre-1772 frontiers for a restored Polish state and his pleas on behalf of the peasantry who were solidly non-Polish throughout almost all the eastern territories of the old commonwealth.

Lelewel's paternal grandfather, Heinrich Loelhoeffel, had migrated from Prussia in the reign of Augustus III. His son, already polonized, maintained close connections with the Polish reform

movement led by Hugo Kołłątaj. Joachim Lelewel, then, was reared in an atmosphere of warm patriotism, which was reinforced later by the influence of Herder and German romantic nationalism. Immensely learned and industrious, he had become in his thirties, while teaching history at the University of Wilno, both the intellectual leader of a generation of young Polish nationalists, which included the poet Adam Mickiewicz, and an acknowledged authority in the burgeoning study of national history. The outbreak of the 1830 uprising against Russia brought him for the first time into active politics: he served as a minister in the short-lived National Government and then exchanged Warsaw for exile in the West after the uprising was suppressed by the tsar's armies. In exile Lelewel took up a left of center position and engaged for a number of years—not very successfully—in émigré politics. His scholarly achievements during this period in the field of history were, however, substantial. He died less than two years before the outbreak of a second, and equally disastrous, uprising in Russian Poland in 1863.

The account given by Skurnowicz of Lelewel's career before 1830 contains much valuable factual information, even if the narrative too often reads as if it were an extended entry in a biographical dictionary. In view of the book's modest size, however, the insertion of a subchapter of eight pages outlining the events leading up to the 1830 uprising appears unnecessary, since most of this may be found in English-language textbooks of Polish history. The account of Lelewel's émigré activities is less satisfactory than the earlier chapters; in this case more background on émigré politics would have been helpful. Perhaps the author was disconcerted by the fact that a thorough and scholarly monograph by Bogusław Cygler exists in Polish on Lelewel as an émigré politician. Cygler also published (in 1963) a detailed study of the Union of the Polish Emigration, an organization that Lelewel helped to found. But Skurnowicz does not use either this or Stefania Sokołowska's monograph on Young Poland (1972), with which group Lelewel also had close contacts.

In short, Skurnowicz's study of Lelewel should prove useful for students of nineteenth-century Poland—but only *faute de mieux*.

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JOHN J. KULCZYCKI. *School Strikes in Prussian Poland, 1901–1907: The Struggle over Bilingual Education*. (East European Monographs, number 82.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1981. Pp. xxii, 279. \$21.00.

This is an even-handed study of an impassioned conflict between the Protestant German governors of Prussian Poland and their Catholic Polish subjects. At issue was the delicate question whether schoolchildren whose mother tongue was Polish should study religion and recite school prayers in German. During the nineteenth century, the Prussian government abandoned piecemeal a confident policy of bilingual Polish education. The Polish nationalist movement that blossomed forth after 1815 aggravated anti-Polish hostilities among German nationalists, who concluded that the Poles must be Germanized, principally through the schools. By 1894, fragmentary Polish language instruction persisted only where religion classes continued to be conducted in Polish. When Prussian officials began ordaining the teaching of religion in German, to justify abolition of all Polish language study, spontaneous resistance welled up, culminating in the "general school strike" of 1906–07, in which 93,000 pupils in 1,600 schools refused to recite religious lessons and prayers in German. Only harsh punishment of the striking students and heavy fines levied on their parents broke their resistance.

The school strikes have always figured importantly in twentieth-century Polish historiography, but John J. Kulczycki's thoughtful study is the best analysis they have yet received. His research was exhaustive and exemplary. His exposition of Prussian school policy is crystal clear. I would only suggest that the root of its radicalism lies in the crisis of the Bismarckian-Wilhelmine political system, whose masters so recklessly sought to shore up their power by conjuring with "anti-German menaces." Kulczycki refutes weakly grounded arguments that the strikes expressed the revolutionary consciousness of Polish peasants and workers. It was, instead, religious sensibilities, often of the most naive and nationally insulated kind, which led students and their parents, especially schoolgirls and their mothers, to resist praying in the tongue of Martin Luther. Fears that Germanization of religious instruction was an opening wedge for Germanization of the Catholic clergy and church service, if not for "Protestantization," were widespread and not always allayed by German nationalist propaganda. But if popular religiosity inspired the strikes and impelled many of the lower clergy to support them, the middle- and upper-class Polish nationalists, especially the ambitious National Democrats, skillfully exploited them to win the "common people's" support for their own ends, which were, whether in the form of Polish home rule or national statehood, fundamentally secular. Not only the government's uncompromising repression of the strikes played into the National Democrats' hands. The uncombative refusal of the primate of Poland and the Vatican to support the strikes, for fear of igniting a new

church-state conflict in Germany, undermined their religious sanction.

Kulczycki concludes that the strikes demonstrate how a popular national consciousness expressed primarily in religious terms could, under the shock of what seemed an attack on Polish Catholicism, undergo translation into conventional political nationalism as the social "elites" defined it. But, as Kulczycki also shows, the strikers' willingness to press their action into the realm of civil disobedience, in contrast to the nationalist parties' legalism, raised the question whether the peasants and workers would long accept subordination to upper-class interests within the national movement.

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A. L. KHOROSHEVICH. *Russkoe gosudarstvo v sisteme mezhdunarodnykh otnoshenii kontsa XV–nachala XVI v.* [The Russian State in the System of International Relations at the End of the Fifteenth and Beginning of the Sixteenth Centuries]. Moscow: Nauka. 1980. Pp. 291. 2 r. 50 k.

This book, completed prior to 1972, has subsequently been revised and updated, and A. L. Khoroshkevich acknowledges the encouragement offered by the late A. A. Zimin. Her study examining Russia's international stature from Ivan III's acquisition of Novgorod to the death of Vasili III (1478–1533) is divided into three units that analyze economic, political, and cultural developments.

In the late fifteenth century Russia exported raw materials like furs and wax, and later leather goods, train oil, fats, flax, hemp, and potash; it imported luxuries, much salt and silver, ferrous and nonferrous metals, and weapons. Foreign agents in such places as Riga, Reval, and the Hanse towns controlled this trade.

The second unit notes that foreign relations did not assume importance until Ivan III had "gathered" the Russian lands. The two grand princes, Ivan III and Vasili III, strove to put together various combinations with other states in their efforts to win back the West Russian lands. Muscovy eventually achieved a form of parity with Lithuania—the capture of Smolensk was offset by the defeat at Orsha. A triumph was the incorporation of Pskov in 1510; the author's contention that the people of that city supported Moscow, however, comes as something of a surprise. Examination of the complex Baltic question chronicles Russia's good relations with Denmark, the decline of the Hanse, and the rise of Sweden. The papacy remained anxious to effect church union after the Council of Florence

and later hoped to bring Muscovy into an anti-Turkish league. Discussion of Russia's relations with the East contains little more than a rehearsal of contacts with the monks of Mount Athos, but it serves as a reminder that Russo-Ottoman relations at this time were stable and friendly. Russia's pragmatic rulers were too shrewd to allow their country to become embroiled in crusades against the Turks.

The final unit confidently asserts that Russia was erasing any cultural backwardness the Mongol yoke had caused. Considerable attention is devoted to Ivan III's marriage to Sophia Paleologa and the coming of the Italians. Greeks in Muscovite service, particularly members of the Trakhaniot family, were skillful diplomats. Transplanted scions of great Orthodox families from Lithuania such as Bel'skii and Glinskii are treated as foreigners. Also provided is a summary overview of other foreigners by profession: mercenaries, bombardiers, architects (Aristotle Fieravanti is preeminent), physicians, linguists, and men concerned with literature (where South Slavic intermediation was marked) and the arts.

This book cites many foreign sources in a host of languages; reference to the primary (as well as secondary) literature is impressive. The treatment is thorough: obscure merchants and other forgotten figures appear in these pages. As is often the case with Soviet works, however, facts do not always support conclusions. The author regularly alludes to Russia's "greatly increased prestige"; other countries vie to acquire "so mighty an ally," and improvements in every sphere of endeavor are startling. The body of the narrative, however, merely demonstrates that Russia exported basic raw materials while importing finished goods, could attain no more than military equality with Lithuania, and was unable to establish unimpeded access to European markets or to break the power of the middlemen. The picture of Russia Herberstein provided was not of a country in which a broad, multifaceted, secular culture was rapidly developing. Perhaps one statement in the author's final conclusion unintentionally comes closer to the mark: "what Russia failed to achieve in the days of Ivan III and Vasilii III was accomplished in the seventeenth and eighteenth centuries" (p. 256).

Nevertheless, Khoroshkevich has written a worthwhile book, even though it may not be exciting reading. Carefully and exhaustively researched, it rivets attention on the fact that the foreign impact on the course of Russian history was felt earlier and was more sustained and capacious than has frequently been assumed. There is an excellent index of geographical terms and Russian and foreign proper names.

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KH. E. PALLI. *Estestvennoe dvizhenie sel'skogo naseleniia Estonii, 1650–1799* [Changes in the Rural Population of Estonia, 1650–1799]. In three volumes. Tallin: Eesti raamat. 1980. Pp. 134; 120; 104. 1 r. 30 k.

This book by Heldur Palli is the first comprehensive study of Estonian population changes in the early modern period, and it is regrettable that it appears in a rotaprint edition of only 600 copies. The work is divided into three parts. The first discusses sources and methodology. Sources for the study of Estonian demographic history are unusually rich. Thirty-nine parish registers are available from the seventeenth century, although due to lacunae in many of them, only three can be used for family reconstitution. For the eighteenth century, researchers have a much larger resource of 83 parish registers, with over thirty being appropriate for family reconstitution. Moreover, greater accuracy and completeness may be achieved for the eighteenth century because of the existence of two additional sources, the local land revisions or *hak* censuses (1712–78) and the Russian soul revisions of 1782 and 1795. These enumerations make it possible to check the highly complicated Estonian system of names and to determine the size and structure of households, including the age structure of the inhabitants.

Much of the first section is taken up in describing and analyzing the problems of family reconstitution. The complicated and shifting personal designations typically used by Estonian registers force researchers to go beyond the family reconstitution forms normally employed in Western demographic history. They must subject these forms to a second stage of analysis known as the system of family identification, a procedure that allows the researcher to determine how many seemingly different families are actually one.

The second part of Palli's study outlines population trends for the period 1650 to 1710. After much turmoil between 1558 and 1628, Estonia enjoyed nearly seventy years free from epidemics and wars (except for the Russo-Swedish war of 1656–58). The population grew rapidly from approximately one hundred twenty-five thousand in 1630 to three hundred seventy-five thousand by 1695. Since the urban component was only about 6 percent, this total represented a remarkably high rural density for the seventeenth century. This population, however, suffered enormous losses in two great catastrophies of the late seventeenth and early eighteenth centuries. The first was the "great famine" that hit the eastern Baltic from about 1695 to 1698. It was made especially bitter for Estonia because of the country's position as a heavy grain exporter. When crop failures hit in 1696 there was little reserve. Two years of poor crops and accompanying

outbreaks of disease cut down between seventy to seventy-five thousand people. As would be expected, the very young, the old, and infirm bore the brunt, or so it would seem, in view of the fairly rapid recovery of the population. But worse was to come. On the heels of the Great Northern War in the early years of the eighteenth century came a terrible epidemic of plague. The scene was prepared by the ravages of the war and a crop failure accompanied by famine in 1709. During the next two years the plague, which had spread from Poland up along the Baltic coast, cut a dreadful path through Estonia's western and northern parishes. Approximately two hundred thousand people perished, well over half the population. Although Palli attributes most losses to the plague, the disease by itself rarely accounts for more than 25 percent of population loss in such epidemics. Famine, war, flight, and other diseases must also have removed a large number.

After these horrors, conditions became unusually favorable for population growth: empty spaces were available, the country remained free of war for two hundred years, and no great epidemics occurred. Section 3 of Palli's work discusses the rapid population growth of the eighteenth century, bringing Estonia's people from one hundred fifty thousand in 1712 to roughly four hundred eighty-five thousand by 1782. The crude birth rate was approximately 40, several points higher than during the growth spurt of the seventeenth century. Nuptiality stood at about 9 per 1,000 in the eighteenth century, slightly below the admittedly very limited indicators for the seventeenth century. Because of the great underregistration of burials (10–20 percent), mortality is impossible to establish with any accuracy. From what is known about the birth rate and rate of natural increase, Palli figured the crude death rate in the eighteenth century at about 27, rising toward the end of the century to 30–33.

Among the many other demographic indices Palli was able to develop were seasonality of marriage, conceptions, and deaths; fertility breakdowns, including for some areas age-specific fertility and the crude and net reproduction rates; and age at first marriage, which in Estonia stood somewhere between the much-delayed nuptiality of northwestern Europe and the low values for Russia and points east. Palli also provides detailed information on household size and structure and employs the Cambridge Group's typology for easy comparison with Western studies.

Only a few of the parish registers are exploited intensively over extended periods; in this sense the study seems to be more comprehensive than it is. But the parishes explored thoroughly are from different regions and are spot-checked against neighboring areas for typicity. Palli provides all

tabular material with English titles and appends a long English summary.

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W. BRUCE LINCOLN. *The Romanovs: Autocrats of All the Russias*. New York: Dial Press. 1981. Pp. xii, 852. \$24.95.

W. Bruce Lincoln, a highly productive young historian, whose contributions to the field include an excellent major study of *Nicholas I: Emperor and Autocrat of all the Russias* (*AHR*, 84 [1979]: 503–04), has now published a huge volume on all the Romanov sovereigns. "This is the story of the Romanovs, those fifteen men and four women who ruled Russia between 1613 and 1917" (p. viii)—even Constantine is included. The eight hundred and fifty pages of text and notes relegated to the back present not only the rulers and their immediate activities but also, extensively, the city of St. Petersburg, to which whole chapters are devoted, and summaries of Russian cultural, intellectual, and social life as well as, more obviously, of politics, diplomacy, and war. The book is a learned and intelligent one with, in general, a fine use of a very rich literature. The author is, broadly speaking, fair and objective, equally ready to praise and to condemn, although perhaps with a slight tendency to extremes in both directions. He writes in a most readable, at times absorbing, manner. The standard of accuracy is very high. The enormous, attractively published volume, with 16 pages of beautiful illustrations, contains but a few mistakes and only a moderate number of misprints—possibly the most galling is the ever-recurring "Schlüsselberg."

Not all readers, to be sure, will approve everything in Lincoln's ambitious undertaking. Some will find the manner of presentation and style frequently breathless, exaggerated in its colors and familiarity, at times even sensationalist. A not untypical sentence reads: "In thousands of villages, the Vanias, Kostias, and Sashas shouldered their rucksacks and bade clumsy, fond farewells to their Tantias, Anniushas, and Matronas, who wept long and sadly waved their kerchiefs, until their loved ones disappeared over the horizon" (p. 684). Others will question the need of such summary sections as the four pages on Pushkin (pp. 492–95) that emphasize his indiscriminate amorous exploits and the claim that he was "the greatest of Russia's romantic poets" (p. 492). A larger structural issue may well be involved here. As already indicated, *The Romanovs* is much more than a mere biographical account of the Russian sovereigns. Yet it is also much less than a

history of the three centuries during which they ruled Russia—indeed, we are constantly better informed about imperial dress and menus than about some of the most important issues of Russian historical development. For example, I consider Lincoln's portrayal of Nicholas II brilliant, but his focus, in terms of Russian history *en grand*, remains very narrow, centering especially on Empress Alexandra and even, alas, Rasputin. Neither simple historical biography nor full-fledged history, every reader will have to decide for himself what he would like to include in it. But that is probably as it ought to be. Readers of this learned journal will, no doubt, continue writing their own books in their own manner, but they should also be grateful to Lincoln for his useful and interesting contribution.

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V. E. ILLERITSKII. *Sergei Mikhailovich Solov'ev*. (Nauchnye Biografii.) Moscow: Nauka. 1980. Pp. 190. 40 k.

The appearance of the Soviet edition of S. M. Solov'ev's *Istoriia Rossii s drevneishikh vremen* (1959–66), generally not reviewed in Western historical journals, testified to the high regard of Soviet historians for Solov'ev. Not until the publication of this volume by the late V. E. Illeritskii (1912–80) of the Moscow State Historical-Archival Institute, however, has there been a Soviet monograph on Solov'ev's life and work. Both Soviet and Western historians have understood poorly Solov'ev's significance in Russian historiography, but this small volume suggests that Solov'ev was the greatest individual Russian historian in the evolution of the national historiography.

This work surveys Solov'ev's life, reviews the development of the historical concepts and principles that guided his work, examines existing literature on Solov'ev, comments on his twenty-nine-volume magnum opus, *Istoriia Rossii s drevneishikh vremen* (1851–79), and analyzes his various monographs. Based solely on printed sources, it makes no claim to being definitive. Partially because no other work of its kind exists, this volume is extremely useful. It surveys both Soviet and prerevolutionary evaluations of Solov'ev's work and specifies essential work remaining for an accurate and complete assessment. Moreover, it provides the best single overview of his magnum opus (now being translated by Academic International Press), giving a general commentary superior to that in the Soviet edition. The author was a specialist in Russian and Soviet historiography, and he claims in his excellent treat-

ment of Solov'ev's historiographical writings, as he did in 1955, that Solov'ev founded Russian historiography as a separate discipline. His analysis of Solov'ev's predecessors in Russian historiography is more detailed and precise than that of his successors, possibly because of the author's own work on the evolution of more radical views in Russian historiography.

Although the sheer bulk and overall impact of Solov'ev's work was widely recognized during and after his lifetime, its full historiographical significance remains undelineated. The author states that the time has come to fill "this appreciable gap in our [Soviet] historiographical literature" (p. 4). He catalogues Solov'ev's numerous achievements, emphasizing the sound factual basis of his publications that resulted from his pioneering archival work. He finds Solov'ev a versatile, dynamic historian, who more than any of his "noble" predecessors or "bourgeois" successors united a profound theoretical understanding with extraordinary productivity. Unless the appeal contained in Illeritskii's last publication is heeded, both Soviet and Western historians will understand the evolution of Russian historiography incompletely, a factor that must limit their own work.

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S. M. MIKHAILOVA. *Kazanskii universitet i prosveshchenie narodov Povolzh'ia i Priural'ia (XIX vek)* [Kazan University and the Education of the Peoples of the Volga and Urals Regions in the Nineteenth Century]. Kazan: Kazan University Press. 1979. Pp. 223. 1 r. 64 k.

Kazan University is often cited by Soviet historians as one of the prerevolutionary centers of opposition to the tsarist regime. S. M. Mikhailova's book, published on the occasion of Kazan's one hundred seventy-fifth anniversary, fits that general pattern. The author adheres to Lenin's division of the liberation movement in Russia into three main stages that correspond roughly to the class leadership provided respectively by the nobility (1825–61), the "bourgeois democrats" (1861–95), and the proletariat (since 1895). She deals with oppositionist thought and action at Kazan University during the first two stages and attempts to prove that the "progressive" teachers and students made real contributions to the education and radicalization of the peoples of the multinational Volga-Kama region. Moreover, she challenges the assertion of M. K. Korbut, author of a standard Soviet history of Kazan University (1930), that, as an organic whole, the university did nothing during the tsarist period to deal with the

problems of the national minorities of that area. According to Mikhailova, it is one of the "urgent tasks" of Marxist historians in an era "when the formerly oppressed peoples of multinational tsarist Russia are participating together in the implementation of plans for communist development," to demonstrate that the progressive and revolutionary elements of *all* nationalities worked together in "mutual respect and trust" to achieve liberation for the tsarist system (p. 6).

The message is clear and the goals are ambitious. Proofs for the thesis are more difficult to adduce. As Mikhailova herself acknowledges, it is difficult even to accumulate, for much of the nineteenth century, trustworthy statistical data on the numbers of Tatars, Chuvashes, Mari, Mordvinians, and others or on their distribution within Kazan's far-flung educational district, not to mention quantifiable data on how Kazan professors and students may have worked with Russia's Finno-Ugrian and Turco-Tataric peoples toward a common goal of an all-Russian liberation (see pp. 16–17, 119–21). Nevertheless, what she has done with considerable skill is to gather from state archives in Leningrad, Moscow, and Kazan many individual cases of cooperation between Russians and non-Russians in the establishment of schools, libraries, and other cultural institutions and, at the covert level, of their cooperation in the organization of secret societies, participation in student demonstrations, and circulation of illegal literature. She is best on the Russian-Tatar contacts, but others are covered. One of the most interesting sections of the book, based on police files, court reports, and university records, shows how individual Polish and Lithuanian exiles, banished to Kazan province in the 1820s or after the uprisings of 1830 and 1863, were gradually integrated into the educational institutions of the province. If the exiles, Russian or non-Russian, had an education, their talents were too precious to waste, and they were put to work in the schools even if their political views were antithetical to those of the higher bureaucrats to whose ministries they were attached.

The book contains excellent reference notes and very good sections on the creation of the Division of Oriental Languages at Kazan University, especially on the political motives behind its establishment, and on the role of the university as a center of general enlightenment in the lower Volga basin.

WILLIAM L. MATHES
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KLAUS FRÖHLICH. *The Emergence of Russian Constitutionalism, 1900–1904: The Relationship between Social Mobilization and Political Group Formation in Pre-revolutionary Russia*. (International Institute of Social

History, Studies in Social History, number 4.) Boston: Martinus Nijhoff. 1981. Pp. x, 349. \$52.50.

This is a confused and confusing book. Klaus Fröhlich, a member of the history faculty at Ruhr University Bochum, attempts to distinguish his study of the background and development of the Union of Liberation movement from the work of George Fischer, Shmuel Galai, Nathan Smith, and Richard Pipes by exploring "the phase of informal group formation during which the question as to how consistent political groups came into being in Russian society is resolved" (p. 6). Aside from the questionable notions of consistency and resolution in his approach, his focus actually differs very little from earlier works. Two of his four chapters concentrate on the origins of the journal *Osvobozhdenie*, the activities of Struve and other "Liberationists" abroad, *Beseda*, and the formation of the Union of Liberation. He uses no significant new material. His thesis, that the transition from "informal" to "formal" organization for Russian constitutionalists "was the reaction of politically conscious sectors of 'society' to the critical confrontation between the outdated autocratic order and a latent revolutionary situation in the country" (p. 234) is hardly new, and, indeed, seems to belie his own stress on the importance of "social mobilization" emphasized in the subtitle. In fact, Fröhlich does not clarify what he has in mind by "the relationship between social mobilization and political group formation." If he thinks there were determinant social structures or processes that explain why Russian "constitutionalism" emerged in opposition to autocratic and radical politics, he does not tell us what they were. We get instead a rather familiar picture of "old informal relationships between several metropolitan groups, in which the tune was called by prominent and influential journalists, scientists, and activists" (p. 241).

The author is also confusing in his conceptualization of "constitutionalists" and "constitutionalism." We learn only through a late footnote that the first refers to those people who can be identified as sympathizers or members of the Union of Liberation; the second term is said to refer to "that section of the range of problems raised by the term liberalism in which parliamentary guarantee of participation became part of the programme of certain social movements in Russia" (p. 11). This muddle is not resolved either by a careful investigation of the constitutional idea outside the Union of Liberation or by analysis of the conflicts within it over the proper nature of a constitution for Russia. More important in terms of the author's professed interests, we are left in the end without any clear understanding of the relationship between Russian social and constitutional development.

This book is a translation from the German original, and this may partly explain the use of terms like "bourgeois-tinged intellectual worker" (p. 81) or "characteristic pungency" of the 1905 revolution (p. 234)—partly, but not entirely.

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TSUYOSHI HASEGAWA. *The February Revolution: Petrograd, 1917*. (Publications on Russia and Eastern Europe of the School of International Studies, number 9.) Seattle: University of Washington Press. 1981. Pp. xxii, 652. \$25.00.

This is the best work in any language on its subject and is essential reading for any serious student of the Russian Revolution. Its greatest strength is lucid, meticulous narrative of the tangle of events in Petrograd (the work is limited to this city, except for necessary excursions to such places as Mogilev) between the onset of disturbances on February 23 and March 3, when the Provisional Government issued its first proclamation and Grand Duke Mikhail Aleksandrovich declined the throne. Tsuyoshi Hasegawa has devoted many years to the mastery of the extensive literature bearing on this period and has added the fruits of his own substantial labors in Soviet archives. He succeeds in dealing with the civilian masses, the radical activists, the liberals, the court and cabinet, and the soldiers and the high command, tracking each sphere in its own path and in relation to the others. In general the story has been told before, but never with such balanced perception and control of pertinent detail.

Hasegawa finds much to sustain in the prevailing Soviet interpretation. He finds the liberals either passive or frustrated in their efforts to conspire with the generals on the eve of the explosion. He credits the ordinary workers with the ignition of the revolution, revising the Western tendency to belittle the impact of the Bolshevik underground as an organizing force. But he differs with the Soviet historians in finding that the *Mezhraiontsy* and the Menshevik-dominated workers' group of the War Industries Committee contributed substantially to the mobilization of radical workers. Although he attaches great importance to the various weaknesses of the tsarist regime, Hasegawa does not consider that its fall was determined until the mutiny in the garrison became widespread and the authorities mishandled what he takes to have been real opportunities to repress the upheaval. He finds that the Duma Committee's control of the railroads and Rodzianko's self-interested misleading of the military played a crucial role

in bringing about the abdication. Indeed, the clarification of Rodzianko's role in the whole crisis is one of the special points of interest in the study.

Any historian of the February Revolution is faced with a dilemma concerning the treatment of the coming of this crisis. One can hardly ignore the problem of understanding the pre-existing problems in Russia, but a detailed consideration of them can lead to the expansion of the study to infinite proportions. Hasegawa has dealt reasonably with this difficulty by devoting approximately the first third of the book to the situation in Russia in 1914–16 and "on the eve." This portion of his study is substantially based on secondary works, and it does not speak with the same authority as the latter sections. It is well informed and judicious but cannot plumb deeply all of the issues that are involved. I was particularly disappointed by the lack of a substantial interpretative discussion on the question of Rasputin's role, actual and as antiregime propaganda. Although the Russian and Western liberal tradition, rooted in the propaganda of Russian liberals of the time, emphasizes the importance of this issue, Soviet historians attach little importance to Rasputin. Martin Kilcoyne, who preceded Hasegawa as a student of Donald Treadgold, wrote an interesting dissertation revising the usual Western view of Rasputin's influence, yet Hasegawa does not mention this work.

Although this book includes illustrations and a useful chronological table, it is seriously deficient in maps. Much of the narrative cannot be grasped without an understanding of various details of the map of Petrograd, but one is provided only with a small one in the end papers. On this scale the print giving street names is virtually illegible, and the Nevskii Prospekt is not labeled, nor are the various canals and bridges that play an important part in the narrative. The reader is somewhat better served by two maps dealing with railroads, although I am unable to find the "Vindarskii line," which has a significant role in the narrative (pp. 440–41). Not the least of Hasegawa's achievements is the writing of a long and complex work in a language that is remote from his native Japanese. On the whole the presentation is satisfactory or better, but his editors should have worked harder to deal with occasional slips in usage.

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KATERINA CLARK. *The Soviet Novel: History as Ritual*. Chicago: University of Chicago Press. 1981. Pp. xv, 293.

Human cultures make myths to justify historical change. Katerina Clark argues that socialist realism, the reigning aesthetic dogma of the Soviet Union, is a myth, in part manufactured by the state, but in part created by the laws of mythmaking. Not for Clark are the pleadings of the Soviet hagiographers who argue that socialist realism is a natural byproduct of the Russian Revolution. But Clark also rejects the oversimplification of some scholars in the West who see socialist realism as merely a set of norms imposed on Russian art by the Communist party.

In Clark's view socialist realism is a response to a problem first debated by nineteenth-century intellectuals and later taken up by the Leninists: Can the people be trusted to act spontaneously, or must the leaders who have the interests of the masses at heart dictate the thoughts and actions of the people? In the 1930s Gorky and his fellow mythmakers found the art of the 1920s too chaotic, abstract, and anarchistic to serve as the basis of the new Communist artistic credo. They had no choice but to turn to nineteenth-century realism as a model and to try to force-feed it with a new mythology. The center of the new canon was the hero as a rite-of-passage figure who moves from spontaneity to the higher discipline of party consciousness. Once the hero has achieved the new *Weltanschauung*, he can join the happy, sternly disciplined family of Communists. In Clark's view, the fundamental parable of freedom versus collective consciousness has haunted not only Stalinist thought but also the "thaws" of the Khrushchev years, the village writers of the Brezhnev era, and, paradoxically, contemporary dissident writers in the Soviet Union and in the third emigration to the West.

Clark's thesis will cause readers to re-examine their views of socialist realism to see if their unconscious, facile assumptions about the movement are warranted. Those readers who have come to realize that history is a branch of mythology will find Clark's book a stimulating and rewarding account of Soviet mythopoeisis.

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NEAR EAST

HUGH KENNEDY. *The Early Abbasid Caliphate: A Political History*. Totowa, N.J.: Barnes and Noble or Croom Helm, London. 1981. Pp. 238. \$27.50.

Despite the imposing contribution of the early Abbasids to the development of Islamic government, modern scholars, as a rule, have shied away from treating the formative years of the dynasty. Recently, however, a number of works have ap-

peared that help redress this imbalance. Among the publications of the last decade or so are specialized studies dealing with the revolution that brought the Abbasid regime to power (M. A. Shaban and Moshe Sharon), internal family politics (Jacob Lassner), the topography of Baghdad (Lassner and Salih al-Ali), and the vast provinces to the east known collectively as Khurasan (Elton L. Daniel). A more general description of the period through the reign of al-Mahdi (d. A.D. 785) is provided by Faruk Omar in both Arabic and English, but his understanding of events is often tied to a literal reading of an extremely complex historiography.

Hugh Kennedy's work is the first serious attempt to paint the formative years of Abbasid rule in broad strokes. His study begins with the origins of the revolution (ca. A.D. 717) and ends with the completion of al-Ma'mun's reign in A.D. 833. Some may argue that the death of al-Rashid (A.D. 809) is a more convenient watershed for the study of the "early" Abbasid caliphate, and that the debilitating civil war that followed shortly thereafter actually marks a new departure. Nevertheless, questions of periodization are always sticky, and there is surely no consensus as regards what constitutes the age under consideration. Kennedy's inclusion of the civil war along with an additional chapter on the reign of al-Ma'mun ("An Age of Transition") has a logic of its own, and, in any case, it will hardly offend anyone who is sensitive to the nuances of early Islamic history.

For the most part, the book is arranged according to chronological developments. The chapter headings of this section are: "The Origins of the Abbasid Revolution"; "Saffah—The Laying of the Foundations"; "Mansur: The Years of Struggle"; "Mansur: The Consolidation of Power"; "The Reigns of Mahdi and Hadi"; "Harun al-Rashid"; "The Great Civil War: I–II"; "Ma'mun: An Age of Transition." This section is preceded by a chapter on geography and is followed by two thematic chapters entitled "Patterns of Provincial Administration" and "Alid Relations in the Early Abbasid Period." The book begins with a terse introduction and ends with a brief note on the medieval sources, which could perhaps be omitted, since it is not particularly informative to scholars working in early Islamic history and would seem to have little purpose for general historians. There are also useful genealogical tables, maps, and a glossary of Arabic terms. A short bibliography of Arabic sources and secondary works is included.

The book is well written, but it strikes this reviewer as a compromise between a textbook and the digest of a doctoral dissertation. On the dust jacket it is described as a work "designed as a unique introduction to a period where existing books are either much broader general surveys, or concentrate on specific aspects. As such, it will prove invaluable to

all students involved with Islamic, Byzantine, and Mediterranean history and culture." It is safe to say that teachers of Islamic history, particularly in the survey courses, will benefit greatly from the publication of this book. There is, nevertheless, a price to be paid for giving "The Early Abbasid Caliphate" general appeal.

Introductory surveys have a tendency to fudge difficult issues when faced with the need for structural conciseness and more clearly established causal relationships. Unfortunately, the highly tendentious sources describing the early Abbasids make this task most difficult. While Kennedy's treatment of the period generally follows the conventional wisdom of contemporary scholarship, there are numerous instances of statements that require, at the least, great equivocation. The proper place to deal with these problems might have been the annotation, but the footnotes that are appended to each chapter seem at best gratuitous. References to the secondary literature are at times haphazard, and they are generally limited to the broader surveys. Moreover, the Arabic sources cited do not always prop up the author's broad contentions. In fields of study where there is considerable sharing of much knowledge, these reservations might appear capricious, but as Kennedy's work will, no doubt, serve as required reading for all students of Islamic history a word of caution is perhaps in order.

There is every reason to agree with the author's own assessment of this work: "Much basic work remains to be done and many problems solved before it will be possible to produce a 'standard work.'" Kennedy modestly concludes his introduction with the hope that both historian and Arabist will derive some interest or enjoyment from his account. He will not be disappointed in this respect.

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MARK ALAN EPSTEIN. *The Ottoman Jewish Communities and Their Role in the Fifteenth and Sixteenth Centuries*. (Islamkundliche Untersuchungen, number 56.) Freiburg: Klaus Schwarz. 1980. Pp. xii, 310.

This study is particularly noteworthy as one of a rare species, a study in Ottoman Jewish history using Ottoman archives. Mark Alan Epstein's archival research, in conjunction with a broad base of secondary literature, indicates that the success in the Ottoman empire of the Jews expelled from Spain in 1492 was less a fortuitous accident than the result of earlier successful Ottoman deployments of Jews to fulfill economic tasks. When the Ottomans moved their capital from Bursa to its first European location, Edirne, they moved certain elements, including Jews, there to develop the city. The clearest

indication of the success of this policy was that the Ottomans repeated it on a larger scale immediately after 1453 in their attempts to restore vitality to Istanbul. Epstein's archival data cast some light on the movement to Edirne and then much more on that to Istanbul and on Jewish economic activity there before 1492, its forms (tax farming, operation of concessions such as docks, commercial and financial ventures), its structures, and consortia of various kinds.

For the two centuries under discussion, Epstein presents a periodization of Ottoman attitudes and policies toward the Jews, in which the reign of Beyazit II, religiously more conservative than his illustrious predecessor, Mehmet II, emerges as a period of narrower tolerance for the Jews. The fact that, despite this, it was during his reign that the Iberian Jews were given refuge and great opportunities becomes another argument for the "building block" approach to the Iberian post-1492 success story.

The history of the Grand Rabbinate of Istanbul is one of initial quick rise to considerable eminence after 1453, and Epstein's analysis of the changing Turkish terminology is informative as to how the Turks viewed the status of this institution. The rapid decline of the institution in the sixteenth century was connected with the rise, in the fifteenth and sixteenth centuries, of what Epstein terms "Ottoman Jewish Politicians." These were Jews whose prominence in the Jewish community was based not on religious leadership but on their influence and connections in the Ottoman world—a type analogous to the *shtadlanim* elsewhere in Jewish history or to the Phanariots in later Ottoman-Greek history. Epstein adds, from archival sources, some data about some of the lesser known of these figures and then integrates further data into the stories of some of those better known, already discussed in secondary literature (in all, a very small group). It seems clear that our knowledge of these figures will grow only through further, similar archival research.

Any frank evaluation of the current state of the historiography of Ottoman Jewry must conclude that the amount of solid data available is so small that talk of "models," great syntheses, and so forth, is most premature. Some readers, coming to this work from more developed areas of historiography, might wish to fault it as being not a multidimensional, conceptualized structure of a community, but rather a montage of pockets of light on a two-century continuum of shadow and darkness. As Epstein mentions, this is essentially an unrevised dissertation, and as such it has some technical problems: some uncorrected misprints, some of the discussion of the secondary literature in the text should have been in the notes, the names of Balkan towns are, in the text, given in their Ottoman form,

unfamiliar to many readers, with the more familiar, usually Slavic names, given only in the tables at the end of the work. Also Epstein appears not to have worked with the Ottoman archive in Thessalonika. After having said this, however, one must take into account the particularly arduous research conditions in the Ottoman archives (an experience the reviewer has shared) that make the winnowing process longer and harder than in Western archives, and one must conclude that it is to Epstein's credit that he has gone this harder route to uncover new data rather than the easier route of facile, fashionable gimmickry. The value of this kind of work will be felt even more keenly in the coming years as the ongoing massive decline of area studies shrinks further the pool of those who have undergone the requisite, long, and multifaceted training here and have served the trying apprenticeship abroad.

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WALTER F. WEIKER. *The Modernization of Turkey: From Atatürk to the Present Day*. New York: Holmes and Meier. 1981. Pp. xix, 303. \$37.50.

Walter F. Weiker of Rutgers University, a political scientist and specialist on Turkish affairs, has written a comprehensive assessment of the first fifty years of Turkey's development (1930–80) within the framework of modernization theory.

Organizationally, the book is divided into nine chapters. The first chapter is an overview of the first half century of Turkey's modernization. Chapter 2 deals with "The Development of Modernizing Leaders," including those on the national and local level, the bureaucracy, the economic elite, the military, and students. In chapter 3, the author discusses "The Modernization of Turkish Followers," namely, villagers, the urban lower strata, and Turkish workers in Europe. The fourth chapter, entitled "Groups and Forces," describes interest groups, the military, religion, and the status of women. Chapter 5 is about the important role of political parties in Turkey's modernization. Unfortunately, during 1978–80 the political party leaders who were involved in factionalism were unable to deal with the escalating terrorism and spiraling inflation. Chapter 6 includes valuable information on "Socialization and Integration: Education, Communication, and the Arts." Chapter 7 is about economic development, and chapter 8 covers government and organization. The last chapter is an assessment of Turkey's achievements and shortcomings.

Among Turkey's main achievements, the author mentions the commitment to maintain a democratic system of government (despite some brief interruptions of a "repair" nature), a sense of national

identity and confidence, the integration of the population into the mainstream of national life, firm rooting of the concept of secularism, and substantial economic growth. Turkey's shortcomings include "far from perfect" social mobility, poor quality of education, an inefficient bureaucracy, regional imbalance, political factionalism, and serious economic problems.

In regard to the outlook for Turkey, Weiker states that it remains to be seen whether the leaders of the most recent military intervention (September 12, 1980) can make structural changes that will restore the basic political vigor with which Turkey solved many of its problems in the past. During the last fifty years, progress was achieved in many fields of endeavor, although the military had to intervene in 1960, 1971, and 1980 in order to restore democracy. The author concludes by stating that there is considerable ground for optimism, because deep roots have been planted for meeting the challenges of the decades to come and because the Turkish leadership, whether military or civilian, continues to pledge itself to a democratic course. Since the publication of Weiker's book, terrorism has been virtually halted, inflation is being brought under control, and a constituent assembly convened (October 23) to promulgate a new constitution.

Despite the serious external and internal threats still facing Turkey, I am in agreement with Weiker that the prospect for Turkey is good. Under the capable leadership of General Kenan Evren, who is considered another Atatürk by most Turks, the challenges of the future will be met successfully. Turkey will remain a staunch member of NATO and of the family of democratic nations.

KERIM K. KEY
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D. EDWARD KNOX. *The Making of a New Eastern Question: British Palestine Policy and the Origins of Israel, 1917–1925*. Washington: Catholic University of America Press. 1981. Pp. vi, 219. \$19.95.

JOHN DARWIN. *Britain, Egypt, and the Middle East: Imperial Policy in the Aftermath of War, 1918–1922*. New York: St. Martin's Press. 1981. Pp. xvii, 333. \$25.00.

The wreckage of the Ottoman, Persian, and tsarist empires presented unique opportunities to Britain in the post-World War I Middle East. These two books demonstrate the continued fascination of the subject, and doubtless there will be others. John Darwin, for example, is disturbed by the commonly held view that Britain rushed into close control of Egypt, Palestine, Iraq, and the Turkish Straits, Persia, and parts of Central Asia, only to be driven out

again by a combination of newborn nationalism and economic and psychological weakness at home. His alternate thesis is that Britain's expanded commitments were only for wartime necessities, and it was natural that Britain return to the general principle of light but firm rule of imperial appendages with a minimum of involvement in internal affairs. Physical resources, like public enthusiasm for empire, were limited, so splendid Cromerian aloofness was always the ultimate objective. In defense of his interpretation, Darwin offers two case studies, each occupying half of *Britain, Egypt, and the Middle East*: Egypt, on the one hand, and the entire Asiatic Middle East, excluding Syria, Palestine, and the Arabian Peninsula, on the other.

Darwin writes well, particularly when summarizing a complex sequence of events. His discussion of Egypt, focusing on the Milner mission in particular, is a model of its kind. The section on the Middle East, however, is too ambitious, and by necessity not everything of importance affecting British policy over the long reach from Chanak to Peshawar can be included. Though they have elements in common, the problems of Turkey, Iraq, and Persia each deserve fuller treatment. To some extent, this shortcoming is overcome by concentrating on cabinet-level politics, where Darwin is at his best, always aware of the personal axes ground by Curzon or Milner or Lloyd George. From the viewpoint of London, his thesis is reasonably sound; despite occasional frantic urges to cut and run, the general pattern into the early 1920s was to hold secure the imperial lifelines. Darwin insists, with some justice, that it was still this motive, not fear of the new nationalisms nor the equally new economic imperialism (of petroleum in particular), that dominated British policy.

Darwin's work is thus a useful corrective to some of the wilder claims of Middle East revisionists. Nevertheless, the cabinet was often rather remote from the daily fears of underlings in the treasury or the foreign office, and still further removed from the desperately small holding forces facing a growing Bolshevik wave in Central Asia or the Caucasus, Arab rebellion in Iraq, and Kemalist rifles in Turkey. On that level, larger strategic concepts could be forgotten, and somehow it was never quite business as usual. Darwin's focus is also his limitation, one stemming from nearly exclusive reliance on cabinet-level papers, official and private, and the published documents.

Darwin excludes Palestine from his study with the weak argument that it excited little controversy among British ministers once the Syrian frontier was settled. A better and more practical reason is simple limitation of space. D. Edward Knox, on the other hand, gives his entire focus to Palestine, and attempts to relate it to a "New Eastern Question"

arising after World War I. This concept, however, is left undefined, and it is never clear whether Knox means the Arab-Jewish confrontation or the larger question of international rivalries in the Middle East, or perhaps both.

Once the terminological facade is pared away, what remains is a chronological discussion of the evolution of British policy in Palestine from 1914 (the title is misleading) to 1925, based, as in Darwin's work, entirely on British documents and published sources. Knox's standpoint is thus also London, but he adds a further complication by attempting as well to view Palestine through the eyes of Gilbert Clayton, who occupied several important posts in Britain's wartime administration and peacetime occupation. Clayton was capable and important, but Knox lays on his admiration with an overgenerous trowel. By the time of his retirement in 1925 (hence the book's terminal date), Clayton by Knox's own admission was merely a figurehead to show the Arabs they still had a friend in the Palestine administration.

The heart of Knox's argument, which in a way supports Darwin's, is that British policy was clearcut from the start: install a Jewish national home as a loyal defender of the route to India. Individual officials in Palestine or Cairo or London might resist, but they were the exceptions to the Samuels and Bentwiches. Knox rejects arguments that British policy was simply hesitant or confused, or honestly attempting to find a compromise between the irreconcilable forces of Arab and Jew. His interpretation needs more demonstration than he offers, however, particularly supporting details of Palestinian policies in such critical areas as immigration, land transfer, or judicial proceedings.

Of the two books, Darwin's is the more valuable. But both are outdated even as they appear in print. This is less of a problem for Darwin, although he has used nothing published since 1976. Knox cites no source published since 1973, and thus has not consulted several important works such as Isaiah Freidman's *The Question of Palestine, 1914-1918* (1973), and Bernard Wasserstein's *The British in Palestine: The Mandatory Government and the Arab-Jewish Conflict, 1917-1929* (1978); when Knox refers to Mark Sykes's biographer, he means Shane Leslie (1923), not Roger Adelson (1975). No explanation for the hiatus between composition and publication is offered, but it does little credit to either author or publisher and seriously undermines the value of *The Making of a New Eastern Question*.

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FOUAD AJAMI. *The Arab Predicament: Arab Political Thought and Practice since 1967*. New York: Cambridge University Press. 1981. Pp. xvi, 220. \$19.95.

In the first part of this book, Fouad Ajami deals with the Arab crisis after defeat in the war of June 1967. He quotes at length from the writings of a few new radicals who were former sympathizers with the older progressive regimes of Nasser and the Ba'th and became their severe critics after the defeat. The author explains that the true revolution that these radicals waited for after 1967 did not take place because the conservative regimes accepted reconciliation with their former rivals—who had just suffered defeat—in the interest of stability and fear of the new radical revolutionaries. At the same time Ajami presents the defense of some Muslim fundamentalists and conservatives against those who attempted to link Islam to the defeat.

In the second part of the book, the author discusses thought and choices among Egyptians and speaks of Egypt, with some exaggeration, as the Arab mirror that epitomizes the limits and possibilities of Arab society. He repeatedly asserts that Egyptian patriotism and the nationalist phase of Egyptian history were discarded under Nasser in favor of Arabism, while the new Egypt of Sadat stands for an Egyptian Egypt. He explains with equal repetition Sadat's motives for the war of October 1973 and describes Sadat's repeated violation of Arab sensibilities, his escapism from the problems of Egypt, and his ambition to make Egypt the sentry of the West in the region. He makes an interesting though not entirely correct parallel between Khedive Ismail and Sadat in their fondness for things Western and their dependence on the West.

In the third and last part, Ajami discusses the questions debated after the war of 1973, the question of Muslim fundamentalism and its roots, in particular, as well as the questions of authenticity and modernity.

The author warns his readers at the very beginning (p. 4) that there is no "fun" in the material he has handled. "It is a chronicle of illusions and despair," he writes, "of politics repeatedly degenerating into bloodletting." He likewise admits toward the end of his book (p. 198) that it may be said he repeatedly and unfairly elaborated the many failures of the Arab order. The truth is that aside from some reasonable explanations and insights, Ajami does more than paint a one-sided, dark, negative, and depressing picture of the Arabs and their politics. Instead of presenting a systematic and objective description of the Arab predicament and an equally systematic study of Arab thought and practice in its various aspects, he offers the reader a substantial number of controversial, often incorrect, and startling statements and indictments. He begins in his introduction (p. 3) with the assertion that the wounds that mattered in the world of the Arabs were self-inflicted wounds, that the divisions of the

Arab world were real and not a colonial trick of divide and conquer. Nowhere does he care to discuss the impact of the tragedy of Palestine, the role of the Western powers since World War I in Arab divisions and tragedies, and the pressures of Israel's supporters on the Arab world before and after Camp David. Equally startling is the author's obsession with the negative effect of Arab wealth and "petro-power" on the Arabs in bringing, as he says, great suffering, dislocation, and unprecedented trouble (pp. 7, 10). He cannot understand how the Arabs talk of autonomy and authenticity while they buy the latest technology and technical projects, conclude financial deals, and use concrete and steel (p. 195).

Among many other exaggerated and incorrect statements are the following: Egypt is the only nation state in the Arab world (p. 104); Nasser gave pan-Arabism its moment in the sun (p. 127), which is actually the other way around, for it was pan-Arabism recently adopted by Nasser's Egypt that gave Nasser his place in the sun. The author thinks that the Palestinian fight in the name of Palestinian nationalism meant that the adherence to pan-Arabism was set aside (p. 123); he also holds that the secular modernized elites dismantled the defenses of local culture (p. 170), and he maintains that when state power is in the hands of the modernized group, war is waged against tradition and belief is undermined.

It is unfortunate that the author has adopted this negative polemical approach with its sweeping and misleading statements, has exaggerated the failures of the Arab system without trying to study their basic causes, and has failed to recognize that the Arabs have been modernizing long before 1967 without breaking with their history and tradition, and what they adopted from the West included the intellectual as well as the material aspects.

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AFRICA

PAUL IRWIN. *Liptako Speaks: History from Oral Tradition in Africa*. Princeton: Princeton University Press. 1981. Pp. xxiv, 221. \$18.50.

Liptako Speaks is a persuasive case study of the uses and pitfalls of oral tradition in historiography, which incidentally provides some political history for an obscure Fulani emirate in northeastern Upper Volta. Although Liptako is a small and not terribly important political unit, it has enough elders with a sense of the importance of their past and

enough contact with other parts of the Sahel that Paul Irwin could obtain considerable information from local sources and corroborate some key points from outside accounts.

Irwin's study tests the reliability of oral traditions in resolving key historical problems, establishing a chronology by sifting accounts of lineages and the successive emirs who ruled Liptako, dating key historical events by tying them to the chronology of rulers, reconstructing the founding of the emirate during the jihad of 1809–10, and providing some historical developments since 1809. Throughout the book, the author concentrates on the evidence offered by different elders and the discrepancies among their accounts.

In the process, Irwin provides solid illustrations of the problems of using oral sources. Beyond the universal problem of distortion creeping in as each generation transmits afresh the events of the past, Liptako provides the further problem that many past events were not considered important enough to be transmitted at all. Although a chronology of rulers and their terms of office seems generally consistent throughout the emirate and accords with documentary evidence, Irwin finds less agreement on local genealogies, and sometimes very little interest in these. Heads of lineages and of villages who "never did anything" were liable to vanish completely from memories, as were non-Fulani neighbors and others who were unimportant in Liptako's cultural context.

Irwin also records more deliberate distortions. The tendency of most elders to put their own clan or family in a favorable light could usually be countered by seeking accounts from all lineages concerned as well as from disinterested third parties. A more difficult problem, however, reluctance to tell outsiders about one's past, was highlighted when one informant cheerfully told Irwin that he and his fellows had "just lied" to a renowned Nigerian scholar who had taught in Liptako for many years and that foreigners could never understand Liptako, comments that Irwin admits left him uneasy (p. 39).

Yet in the absence of more reliable sources, what can a historian do? Irwin cautiously concludes that, although oral history cannot resolve problems of detail, it can illuminate the broad contours of nonliterate societies, provided it is used with care and patience. His experiences are useful ones, and researchers of oral traditions would do well to bear them in mind.

JOHN CARTWRIGHT
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GAVIN KITCHING. *Class and Economic Change in Kenya: The Making of an African Petite Bourgeoisie, 1905–*

1970. New Haven: Yale University Press. 1980. Pp. xx, 479. \$35.00.

As his subtitle suggests, Gavin Kitching is largely concerned with one facet of class and economic change in Kenya: the making of an African petite bourgeoisie. He wants the book to be "judged above all, by Marxists, as a work of Marxist theory" but hopes that it will be of interest to others. As one of the "others"—a historian who has sometimes blundered into agricultural economics—I have no doubt that the book will be of much interest to historians, Marxist or otherwise, even though Kitching has not succeeded, as he admits, in bringing together history and Marxist theory "in a fashion which would make their interdependence obvious" (p. 6). Nevertheless, the book covers more than the making of a petite bourgeoisie: indeed it is the most comprehensive study yet published of the African economy in Kenya. Previous scholars have been content with monographs on segments of that economy: with land tenure reform, resettlement schemes, cash cropping, wage labor, and business. Kitching discusses all of these and more, including a brave attempt to examine the "informal sector"—that part of the African economy not encompassed by official statistics.

The first two parts of the book are devoted to the period 1905–52, under the somewhat constricting title "Some Economic Origins of Mau-Mau"—constricting because Kitching ranges far beyond the Kikuyu tribe that was the main participant in Mau Mau. On the way, he makes some valuable points: that European agriculture, usually regarded as a hindrance to African agriculture, stimulated African cereal cropping (to feed African laborers) and that African wages and business profits often provided capital for purchasing and developing land in the reserves. Nevertheless, the overwhelming impression is of a gathering crisis, not just in Kikuyuland, but in other heavily populated reserves as well, beginning in the interwar years but intensifying afterward in the unrest that culminated in the Mau Mau rebellion. It was a crisis exacerbated by a growing division between landed and landless. By 1952 Kitching's petite bourgeoisie was already in existence, having formed around a coterie of chiefs, headmen, small businessmen, and petty officials.

As Kitching goes on to demonstrate in the remaining part of his book, the so-called "agrarian revolution" that followed the outbreak of the Mau Mau emergency did nothing to alter this basic class formation. I would agree, at least from the more limited focus of my study of Kikuyu land reform. But Kitching goes further with some important concluding remarks. Kenya's petite bourgeoisie had not become self-sustaining: "The essential contradiction facing the Kenyan petite bourgeoisie is not

the product of their country having been too exploited by capitalism, but . . . of its not having been exploited enough." Either it must "attempt to carry through to its conclusion a capitalist revolution in production there, or . . . remain an increasingly indebted appendage to the world system . . ." (p. 430). Colin Leys has convincingly demonstrated the latter condition in his *Underdevelopment in Kenya*. But so far, as Kitching demonstrates in a final chapter, Kenya's petite bourgeoisie has found a satisfying alternative to a full-blooded capitalist revolution: it has been succored by the state—by an expanding bureaucracy and by producer boards—at the expense of the peasants and the proletarians. For with independence the petite bourgeoisie (who include Kenya's *nouveau riches* politicians and civil servants) captured the machinery of the state and turned it to their advantage.

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MARGARET STROBEL. *Muslim Women in Mombasa, 1890–1975*. New Haven: Yale University Press. 1979. Pp. xiii, 258. \$19.50.

The historical study of women in Africa has received far less attention than that of women in Europe or America. This pioneer work, a winner of the Herskovits Prize in African Studies, is an important contribution to research on women and merits the attention of anyone interested in the interconnections between sex, class, and ethnicity. It thoughtfully portrays the changing relationship between a female subculture and the wider society in which it exists, exploring both the strength of that subculture and its lack of an explicitly feminist orientation.

Margaret Strobel's first chapter, a discussion of wedding ceremonies, explores one of the book's central themes—the cultural interaction between women of slave and free ancestry. The second chapter, on the history of the Swahili coast, provides the background for her analysis of the following topics: slavery and male-female relations, the impact of Western education, the marginalization of women's work, and women's collective organizations.

The book's findings in some areas confirm the conclusions of other studies about women and, in other areas, add interesting and original perspectives to our current knowledge. Strobel explains the differential impact of Islam on women of different classes and shows how women in Mombasa, as in many other parts of the world, have compensated for their exclusion from public affairs by participating in unorthodox non-Islamic rituals such as spirit possession cults. She also demonstrates the way in which marginalization within the formal sector of

the economy, particularly for lower-class women, has led to their concentration in more independent occupations. Discussing the distinct female institutions that are more specific to Mombasa, she clarifies the ways in which particular organizations (dance societies, "improvement" associations, and initiation groups) have drawn on varying traditions and varying class and ethnic interests to fulfill the needs of women in ways that sometimes succeeded in transcending the barriers that divided them from each other.

The book is well written, insightful, and sensitive both to feminist perspectives and to the concerns of the women whose lives it depicts. My only reservation relates to its lack of a clearly stated thesis that might have developed the theme of differentiation on the basis of class, sex, and ethnicity in a more systematic and theoretical way, thereby giving the book more cohesiveness. The sections on education and work, for example, do not indicate clearly whether new opportunities in these areas tended to reinforce or to diminish existing class and ethnic boundaries. Interesting insights, such as the explanation of why upper-class women supported slavery (p. 94), are not drawn upon in later chapters either to explain the current traditionalism of elite women or to clarify the extent to which their attitudes toward class have changed. Finally, since the book draws frequently on comparative data, reference to the role of women's institutions in other Muslim societies would have provided a context for evaluating the relative uniqueness of the female subculture of Mombasa.

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JOHN ILIFFE. *A Modern History of Tanganyika*. (African Studies Series, number 25.) New York: Cambridge University Press. 1979. Pp. xvi, 616. Cloth \$79.50, paper \$19.95.

This large, fully packed nationalist history of Tanganyika is a monument to a school of historiography that flourished in Dar es Salaam in "the first exciting years of 'independence,'" when John Iliffe was a key member of the university's history department. The book begins with a survey of the area of the future Tanganyika as it was about 1800, announces economic and cultural themes, and goes on to outline the commercial revolution of the nineteenth century before the German conquest. As a study of the colonial experience and nationalist movement, it reaches its zenith; Iliffe knows the country and its colonial situation very well. He is a first-rate synthesizer who draws on a mass of literature, much of it produced by students, research associates, and staff

of the University of Dar es Salaam since 1964. One might quarrel with the overall conceptualization and explanatory purposes of the work, but each chapter is clearly set out and lends itself to comparison with contemporary colonial states in formation.

The primary organizing purpose is to provide a comprehensive background and account of the social, economic, and political elements that combined in the 1950s in the Tanganyika African National Union (TANU). Understanding this, the criteria and emphases became clearer, for TANU included men from the peasant cooperative movement in the Kilimanjaro and Buhaya coffee zones and the Sukumaland cotton-growing area, both of which receive extended treatment. TANU was led by an educated elite that favored universal democracy and centralism over the tribalism promoted by the administration as the foundation of local government. The front also contained popular Muslim brotherhoods and organized labor as well as Christian schoolmasters and clerks. Each of these elements had its own prehistory, which Iliffe tries to incorporate in the chronologically appropriate places. Taken as a whole rather than as a series of individually illuminating chapters, the book works best when the chapters are read in reverse order.

A second preoccupation is an important one at the forefront of contemporary historiography in Africa, that is, the relationship of man and nature through time. Tanzania is poor, and the adversities of its environment and limited resources have borne down heavily on the people. Their struggles with nature, the book suggests, have been as heroic, if often less successful, than the nationalist political movement. Implicitly, the failures of the state are to be explained by the weakness of the economic base, not by class interests or systematic underdevelopment. Whether or not readers accept this interpretation, they will find riches in the materials put forward in the narrative and are challenged to delve deeper than radical scholars sometimes have to identify the "laws of motion" of capitalism in mainland Tanzania. Iliffe is a scholarly counterinsurgent against official rhetoric, too, and ends his book with a line commenting on President Nyerere's assertion that the government and the people were identical and that together they could turn to mastering nature. Sobered by the disillusionment about Tanzania in the 1970s, the author concludes simply: "But it was more complicated than that."

Just as Tanzania has outgrown the euphoria of its initial independence, so it seems that Iliffe has now done his service for nationalism and is on the way to the practice of a larger, more Braudelian historiography. Meanwhile, we must be grateful for this capstone of the loyalist Dar es Salaam school.

MARCIA WRIGHT
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T. H. ELKISS. *The Quest for an African Eldorado: Sofala, Southern Zambezia, and the Portuguese, 1500–1865*. Waltham, Mass.: Crossroads Press. 1981. Pp. 121. \$12.00.

At the beginning of the sixteenth century the Portuguese crown received assurances from colonial officials of Mozambique's enormous potential. The jewel of the Portuguese East African empire was the coastal town of Sofala, which "is and will always be one of the best things Your Highness has here, albeit it might not seem so now" (p. 69). Three hundred years later a British naval officer stationed off the Sofala coast observed that "notwithstanding the progress of science and discovery, this country remains, in 1812, in that happy state of obscurity in which it was founded by Vasco da Gama" (p. 61). And he was being generous. After three hundred years of nominal Portuguese rule, Lisbon's presence at Sofala was limited to a crumbling stockade and a handful of former convicts and social outcasts, all of whom had become Africanized.

The Quest for an African Eldorado is the story of Lisbon's failure to transform Sofala and the southern Mozambican hinterland into a prosperous colony. It is a story that actually began several hundred years before the arrival of the Portuguese, when, T. H. Elkiss argues, Arab geographers, explorers, and merchants incorrectly portrayed southern Mozambique as a major gold-producing region. Over time the Portuguese came to believe that the East African hinterland was the home of the legendary mines of King Solomon. For Lisbon, control over Sofala and its hinterland offered a double attraction—gold to help finance the maritime expansion and a strategic base from which to drive the Muslims out of the Indian Ocean as well as the interior. Between 1505 and 1530 Lisbon imposed its hegemony over the coastal zone, setting the stage for a recurring comedy of errors. Over the next three hundred years, unqualified and underpaid local officials generally working in collaboration with undercapitalized merchants made repeated forays into the interior. They neither discovered the mythical mines nor effectively incorporated the region into the Portuguese colonial empire. All that remained was the crumbling stockades at Sofala.

In a curious way, Elkiss's study is the final victim of the Portuguese failure. The author had originally planned to write an urban history of Sofala. One could imagine an exciting study of the process of urban growth and social differentiation that accompanied the incorporation of Sofala and its hinterland into the world economy. From a slightly different perspective an analysis of the patterns of cultural change that accompanied the fusion of Shona, Arab, Indian, and Portuguese traders and settlers would have made a fascinating study carry-

ing important comparative implications. Unfortunately, Sofala neither experienced the growth nor assumed the commercial significance of Mombasa, Malinda, or even Mozambique Island. Throughout most of the period it remained marginal and largely disconnected from major changes taking place in the Indian Ocean economy.

To Elkiss's credit, he attempted to transcend the insignificant details of the Sofala debacle by placing his study in the broader setting of a southern Mozambican regional economy. Two weaknesses, however, limit the value of this endeavor. Because Sofala and the hinterland were backwater areas, primary documentation is extremely fragmentary. Difficult political conditions during the colonial period and Rhodesian attacks after independence precluded fieldwork that might have added valuable data. As a result, the author's discussion of the inland trading states such as Manica and Quiteve is not very well developed, and his description of the better-known kingdom of the Muenemutapa relies heavily on secondary literature. Moreover, the lack of either a clearly defined thesis or a set of broader theoretical questions reduces the study to a mere narrative of events over a three-hundred-year period. Analysis of such fundamental issues as how labor was organized in the gold mines, the operation of the inland fairs, or the underlying weakness of Portuguese merchant capital would have enhanced the overall value of the study. In the end, the reader is left with the recurring image of the crumbling fort of Sofala and little more.

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BELINDA BOZZOLI. *The Political Nature of a Ruling Class: Capital and Ideology in South Africa, 1890-1933*. (International Library of Sociology.) Boston: Routledge and Kegan Paul. 1981. Pp. xi, 384. \$40.00.

In 1890 *imperial* capital was revolutionizing South Africa and "consisted mainly of mining capital, merchant capital, British-based manufacturing capital, and agricultural capital," with mining "the leading one." By the 1930s *national* capital "made up of industrialists, traders, certain mining companies" and led by "manufacturing" was dominant (p. 171). These various "sections" of capital did not, however, always subordinate the "narrow state" (the system of government), but by the 1930s an English-speaking national bourgeoisie controlled the "wide state" ("the entire system through which the ruling class's dominance is exercised" [pp. 215, 281]). Shifts in the nature of capital and its ability to subordinate and organize labor to its needs through the proletarian-

ization of blacks and the exploitation of whites' race consciousness was, Belinda Bozzoli asserts, achieved in large measure through the activities of "organic intellectuals." These are journalists who were "not mere propagandists for the bourgeoisie" but "an essential part of its existence, consciousness and realisation as a class" (p. 11).

This is not vulgar Marxism, but neither is it elegant Marxism. Capitalist groups are not seen to be simply monolithic and conspiratorial. Indeed, the author chides other Marxists for not formulating sophisticated hypotheses. But the labels used in elaborate categorizations to convey complexities are ungainly, Marxist terminology is not always used rigorously, sentence structure is often convoluted, and even the typewritten print is not professionally done (see especially pp. 132-33).

Business journals are "the basic evidence for the argument sustained in [the] work" (p. 15). But no quantitative techniques were employed in content analysis. No circulation figures are given. Very few of the "organic intellectuals" are fleshed out. Very little use is made of the major dailies to help delineate mainstream class attitudes. Were there no Afrikaner spokesmen representative of agricultural capital? Such omissions underscore the vexing problem familiar to critics and writers of public opinion studies. What credence should be given to editorial comment in shaping and reflecting the group it purportedly represents? What is indiscriminate puffery by eccentric editors? Given the nature of the evidence and the methodology employed, the analysis is impressionistic. A second source problem stems from the fact, mentioned by the author (p. 24), that few empirical studies of South African capitalism exist. The development of the iron and steel industry is a case in point (pp. 217, 339). So Bozzoli cannot be very precise about the stage attained by a particular section of capital at a certain time.

Despite its methodological faults, the book's typology does contribute to a central discussion in South African historiography: the emphasis to be given race on the one hand and class on the other as determining forces in the country's recent past. Not only should it lead to more nuanced work by radical historians, but non-Marxists who emphasize the pervasive function of racism in shaping South Africa's torments should also find her approach illuminating.

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ASIA AND THE EAST

W. J. F. JENNER. *Memories of Loyang: Yang Hsüan-chih and the Lost Capital, 493-534*. New York: Clarendon

Press of Oxford University Press. 1981. Pp. xii, 310. \$69.00.

During the fifth and early sixth century North China was ruled by a non-Chinese dynasty, the Northern (T'o-pa) Wei. For nearly a century the country was ruled from northern Shansi. In 493 Emperor Hsiao-wen (ruled 471–99) moved the Wei capital several hundred miles south to Loyang, the site of several earlier Chinese capitals. This move was made at great cost, not only in money and labor, but also in political support from the Hsien-pei warriors whose ancestors had founded the Wei state. In 523 these warriors rebelled, beginning a decade of warfare that thoroughly altered the political leadership of the country and eventually led to the abandonment of Loyang. According to the historical records, Loyang's two million residents were forced to move to the new capital in Hopei and even many of Loyang's palaces were dismantled and moved.

Wei Loyang, thus, was an unusual city in the perspective of world history. It was created by political decree, quickly became an active center of religion and commerce in addition to government, and then disappeared almost entirely when ordered to do so, all within the space of a single lifetime.

The *Record of the Monasteries of Loyang* (*Lo-yang ch'ieh-lan chi*) is the major source for our knowledge of the splendor of Northern Wei Loyang. Yang Hsüan-chih, an official who had served there, wrote it from memory about fifteen years after the destruction of Loyang. Although the *Record* is organized around monasteries (many of which had been mansions until 528), it is concerned less with Buddhism than with the great events that occurred in the city. The stories used to annotate the descriptions of the temples most often concern princes, generals, and leading figures at court.

W. J. F. Jenner has provided a full and careful translation of the *Record* in part 2 of *Memories of Loyang*. In part 1 he has written a study of Northern Wei Loyang that draws heavily on the *Record* but also makes full use of other available primary sources and the extensive secondary literature on the period in Chinese and Japanese. Jenner is particularly good at narrating the complex political history of the Northern Wei. He describes in detail the sinification program that led to the decision to move the capital and the continually shifting court politics and military campaigns that followed the move. *Memories of Loyang* is thus a major addition to the extremely small number of books in English on the Northern Dynasties.

The translation of the *Record* is a great service to specialists in medieval Chinese history, especially since Jenner provides extensive annotation, paragraphing, and an index. Even with these aids,

however, general readers and students are likely to find the translation difficult to read because Yang Hsüan-chih's organization by location leads to constant changes of subjects. Such readers, however, have much to learn from the study of Loyang in part 1.

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JOHN D. LANGLOIS, JR., editor. *China under Mongol Rule*. Princeton: Princeton University Press. 1981. Pp. xvi, 487. Cloth \$30.00, paper \$12.50.

The present volume, like the 1976 conference from which it issues, addresses itself to the impact of Mongol rule on China—in short, the central question for Chinese historians in their study of any conquest (that is, alien) dynasty. The eleven papers range widely over politics, thought, art, religion, and drama. Seven essays treat aspects of high culture (the “great tradition”), while the other four deal with the Mongol regime, its policies, and its foreign clients. The results of so substantial a volume, contributors to which include some of the leading authorities in the field, are entirely too rich and multifarious to be adequately summarized here. Two important general conclusions do, however, emerge from this collection of studies as a whole. One is that several of the stereotypes commonly held of Mongol rule and its effects must be re-examined and revised. A second, and in part a corollary to the first, is that Chinese culture not only persisted under the Mongols but in many ways was even enriched by the experience. Of the challenges made to established views, David M. Farquhar's is the most radical. Yüan imperial government, he argues, was so structured and staffed as to administer the metropolitan province rather than the empire at large, and the oft-claimed centralizing tendencies of this system simply did not exist. Hok-lam Chan at least partly undermines the disdainful view held of the Mongols as preservers of the historical record by demonstrating the complexities that lay behind the compilation of the Liao, Chin, and Sung dynastic histories. Heavy reliance on other foreigners was by no means free of problems, as the studies by Morris Rossabi of the ambivalent Mongol policy toward Muslims and by Herbert Franke of the favored Tibetan monks show. Moreover, Mongol domination did not, as is often claimed, lead to the rise of drama, strong traditions of which already existed; rather, as Steven H. West argues, it led to elite involvement and to the transformation of drama into a literary art.

It is through the adaptations to new conditions by individuals—that is, through biography and collective biography—that the resilience of the high cul-

ture is demonstrated. In parallel treatments of regional centers (both in Chekiang), Chu-ting Li and John D. Langlois, Jr., trace developments among the artists at Wu-hsing and the intellectuals at Chin-hua, respectively. At the former, a vibrant artistic movement took shape, liberated from Sung academic conventions and enjoying Mongol favor; at the latter, thinkers infused Chu Hsi Confucianism with a strong admixture of utilitarianism. A practical response of intellectuals to alien rule was participation in private academies, whose development is examined by Yan-shuan Lao. Additional studies of value are contributed by K'o-kuan Sun on southern Taoism, David Gedalecia on the philosopher-classicist Wu Ch'eng, and Marilyn W. Fu on the celebrated calligrapher Hsien-yü Shu. The case for the vitality of the "great tradition" is thus forcefully made; but one must ask, What about the great mass of the people whose fortunes appear here only peripherally? While we should not expect too much of a single project (and the necessary materials are scarce to boot), we are obliged to recognize that these papers concern but a thin segment of the nation as a whole. The volume is well composed, carefully edited and indexed, and features characters in the text and useful illustrations (but no map). A useful, brief essay by the editor introduces the collection.

CHARLES A. PETERSON
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JERRY DENNERLINE. *The Chia-ting Loyalists: Confucian Leadership and Social Change in Seventeenth-Century China*. (Yale Historical Publications, Miscellany, number 126.) New Haven: Yale University Press. 1981. Pp. xix, 389. \$35.00.

The special attention to design and printing that Yale University Press has lavished on the production of this handsome volume is merited by the importance and quality of the study that Jerry Dennerline has written. The first part of the title, which refers to the Chia-ting loyalists, tells us little about the scope of the work, and the second half of the title, which refers to Confucian leadership and social change, still does not suggest the breadth of what is dealt with. Following the collapse of the Chinese Ming dynasty in 1644, Chia-ting, a county seat near the mouth of the Yangtze River, was the scene of a brief and ineffectual stand by a handful of idealistic Confucians resisting the inevitable onslaught of the Manchu Ch'ing pacification. Dennerline uses this incident as a unifying device around which to organize a wide-ranging examination of the Chinese scholar elite over a two-hundred-year period. The result is a tour de force in which the same events are viewed from multiple perspectives.

At the outset, in a brief historiographical essay, he rejects both traditional and modern interpretations of the events at Chia-ting. Then, from the perspective of political history, he leads us through the formation of the fundamentalist Confucian Tunglin party in the sixteenth century to its successor, the Restoration Society (*Fu She*), in the early seventeenth century and on into the factional intrigues of the early Ch'ing reigns. The continuities in personnel and factions that Dennerline demonstrates here are by themselves an enlightening account of the Ming-Ch'ing transition, but there is much more. We are given a brief but lucid sample of institutional history in the account of how patronage worked in the structure of academies, bureaucratic organs, offices, and examinations, which constituted the path to upward mobility in the government of late imperial China. There is intellectual history in the description and evaluation of the loyalists' views of morality and statecraft against the background of orthodox Ch'eng-Chu *li-hsueh*, the radical departures associated with the Wang Yang-ming school, and the turn toward empiricism that prevailed in the early Ch'ing. There is regional analysis in the characterization of the effects of increasing commercialization of agriculture and of Chia-ting's links to the surrounding area: the Yangtze, T'ai-ts'ang, Soochow, and Nanking. There is social history of a high order in the reconstruction of family connections, elite networks, and patterns of local mobilization and conflict. There is biography as well in the portraits of the central figures in the tragic and suicidal loyalist resistance. This short list should suffice to suggest the great complexity of the story that Dennerline tells. He is able to tell it and to saturate it with facts, because he spent more than ten years working on it. The reader should allocate a correspondingly generous amount of time to study the resulting eleven chapters of often tedious argumentation and details.

Dennerline's central theme of loyalism serves to throw light on some important issues. By taking Confucian notions of renewal and loyalism seriously, he is able to argue that following commercial development in the delta the Confucian elite with its national connections and aspirations were barred by their ideology from linking effectively to local powerholders. The loyalists were opposed on principle to both local violence and the Manchu takeover. They were capable of symbolic protest but not of effective representation of local interests against the center. It is an argument that recalls some of the conclusions of Max Weber's pioneering inquiry into the "religion" of the Chinese elite. Jerry Dennerline, by analyzing a specific event in great detail, is able to reveal the complexity of local, regional, and national elites and the great variety of values and motivations that operated at any given time. He has written a

major book that opens up innumerable questions and that sets a formidable standard for other researchers to match.

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THOMAS E. EWING. *Between the Hammer and the Anvil? Chinese and Russian Policies in Outer Mongolia, 1911–1921*. (Indiana University Uralic and Altaic Series, number 138.) Bloomington: Research Institute for Inner Asian Studies, Indiana University. 1980. Pp. vi, 300. \$20.00.

Using recently available Mongolian, Russian, and Chinese sources, Thomas E. Ewing has written a history of the birth of the modern Mongolian republic that is closely documented and free of national and ideological biases. The need for such a volume has long been evident, but there are not too many Western scholars who are both interested in this remote Central Asian nation and conversant with the language required to undertake the task of writing it. This book should thus be welcomed by those who need a reliable reference on this crucial period in modern Mongol history.

A major focus of Ewing's work is on the political maneuvering between Russia and China in response to the movement of the Mongolian ruling elite for national independence. As shown by the author, both the tsarist and Soviet governments were adept at seizing opportunities to create a dependent buffer state on their Siberian border. There was no blueprint drawn for such a venture, but events propelled the extension of their power southward. The Chinese authorities were seen as tenacious in their attempt to retain control over the Mongols but inept in the implementation of their policy. In this regard, perhaps Ewing could have stressed the unwillingness of Chang Tso-lin, the Manchurian warlord, to cooperate with the Peking government in effectively maintaining the Chinese presence in Urga.

Ewing proves that the Mongols were not passive actors in the drama of imperialistic rivalry between their two big neighbors. Indeed, they initiated the move to use Russian power against the Chinese for their own national purpose, but they were unable to achieve their goal of creating a greater Mongolia that would embrace all Mongol tribes or even the lesser goal of formal independence instead of autonomy. This is, of course, the fate of small nations dependent on external powers for the realization of their national objectives. Mongolia was just another pawn in Russia's power play against China and Japan in the Far East. As events unfolded, the Russian Civil War spilled into Mongolia and the destiny of the Mongols became inextricably entan-

gled with the fortunes of the Bolsheviks. The nomadic Mongols were in a few years to become Moscow's first satellite nation without the majority of the Mongols having the faintest idea of what communism meant. To this reviewer, the last three chapters, describing the beginning of the Mongolian People's party in 1920 and its victory over the Chinese garrison and the old Mongol elite in 1921, are the most interesting part of the book, particularly because of Ewing's success in restoring the image of those leaders who were later maligned by their erstwhile comrades.

Ewing's concluding remark, in which he states that Mongolia might be a Chinese province today if China had accepted the Soviet proposal of independent or joint military action in Mongolia against the White Russian troops of Ungern-Sternberg, seems to me rather questionable, because the succession of Chinese governments in Peking and Nanking were really too weak to suppress the growing Mongol nationalism. Furthermore, the Japanese were waiting for a chance to expand from Manchuria into Mongolia. The power vacuum in Mongolia could be filled only by Russia or Japan, not by China.

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JAMES J. LORENCE. *Organized Business and the Myth of the China Market: The American Asiatic Association, 1898–1937*. (Transactions of the American Philosophical Society, number 71, part 4.) Philadelphia: American Philosophical Society. 1981. Pp. 112. \$10.00.

James J. Lorence's detailed account of the American Asiatic Association takes the source of innumerable footnotes and makes it the text. No one writing on American China policy fails to cite the association's publications or the correspondence of its major figures. Yet until now the association itself, its organizational framework and history, has remained murky. Lorence's study is a most useful corrective. He traces the association's rise and fall, from its founding in 1898 to its senescence in the 1930s, and in so doing offers a quick review of American-Chinese relations in this period. The first chapter is a succinct setting of the stage in which Lorence reviews the state of the American economy in the 1890s and the remedies prescribed by those who sought to resolve domestic crisis through foreign empire. It's the usual cast of characters—Brooks Adams, Mahan, Roosevelt, and others—but Lorence summarizes their thoughts with considerable skill and makes clear the relationship between their work, the role China was meant to assume in

America's drama, and the organization of a formal lobbying group in 1898.

Of most interest is his analysis of the shifting nature of business involvement in China, from export-import merchants (with their direct ties to cotton manufacturers) to finance capital and large corporations. The effect of the shift on the association itself was profound, and the unwillingness of its aging executive committee to give ground ultimately weakened the role the association could play in influencing policy. Lorence also explores the extent to which an early sympathy for Japanese ambitions in China (and a confidence that cooperation with Japan would enhance American interests) rendered the association remarkably insensitive to the realities of the situation in China—or Japan for that matter.

Chinese realities appear only irregularly in this study, which is not surprising given its topic. In the main, Lorence's observations on the Chinese scene are judicious, though I have a personal aversion to referring to Chinese as "natives," and feel strongly that Chinese "hostility towards foreigners" was not a "national characteristic" (p. 8) but a most comprehensible response to foreign behavior in China. The strength of the myth of the China market, the "money to be made and peace to preserve," as one of the association's executives put it in 1937, is remarkable, and Lorence's work enables us to examine the myth once more through the glass of its earliest organizational expression.

MARILYN B. YOUNG
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MERLE GOLDMAN. *China's Intellectuals: Advise and Dissent*. Cambridge: Harvard University Press. 1981. Pp. 276. \$22.00.

In this exemplary study of recent and contemporary history, Merle Goldman has set herself a very difficult task. She has plunged headlong into the tangled thicket of China's intellectual and political life between about 1959 and 1979 and has sought to explain the relationships between major bureaucratic factions and their scholar-allies. At the same time she places this seeming free-for-all against the entire background of traditional Chinese thought and politics. Fortunately, Goldman is an experienced traveler in these woods, having made notable contributions in her earlier writings. Undaunted by documentation that is at once voluminous, highly partisan, and riddled with gaps, she has picked her way carefully around the hazards and written a highly valuable book. Although she has stayed prudently within her evidence and risked no sidetracks into territory beyond China, her book touches issues of general and even universal concern. It deserves to be read by a wide audience.

The central theme of the book is the struggle

between two political factions and their attempt to employ groups of intellectuals to serve their political ends. Alongside this all too familiar phenomenon, Goldman deftly reveals that there has been a corresponding attempt by intellectuals to use the politicians. Lacking a public forum for dissenting ideas, Chinese intellectuals have repeatedly taken advantage of the media campaigns waged by political factions in order to express a variety of opinions, including many not sanctioned by their bureaucratic sponsors. Time and again the intellectuals have gone beyond the bounds set by the political contestants, setting off tugs-of-war between intellectuals and politicians. We have, in other words, a four-cornered battle among two political factions and two groups of intellectuals. The latter, categorized somewhat questionably as "liberals" and "radicals" by the author, have been losers no matter which political faction came out on top, because in China there is still a powerful tradition that a regime's legitimacy requires at least the appearance of consensus. But no consensus has lasted very long, and each time political factionalism erupted the intellectuals gained a new opportunity to speak out. How they have done so, with remarkable ingenuity and awesome courage, while staying within Marxist categories and stretching ideological constraints up to and occasionally beyond officially approved limits, makes a fascinating, instructive, and sometimes grim story.

Negotiating the ups and downs of the Great Proletarian Cultural Revolution, Lin Biao's plot to assassinate Mao Zedong, the rise and fall of the "Gang of Four" and of their successor Hua Guofeng and his successor Deng Xiaoping, and carrying the analysis down to events as recent as the closing of Democracy Wall in December 1979, all the while balancing her treatment of the political factions and intellectual groups and relationships among them, is a feat of superior scholarly acrobatics on Goldman's part. To complain that she may oversimplify the picture by sticking so closely to her categories of liberal and radical (and even politicians and intellectuals) is perhaps to cavil about bookish matters. But so much writing on recent Chinese politics hews to a "two-line" analysis that we need to remind ourselves of the broad and fuzzy spectrum of opinion existing in China. The publications on which Goldman perforce had to rely give us only a peek through a narrow aperture into that colorful world.

The author's conclusions are cautiously optimistic. The regime's need for intellectuals in the effort to modernize China has finally introduced new factors into the ancient struggle between politicians' stress on unity and intellectuals' thirst for diversity. With Mao Zedong gone and the authority of the party steadily eroding, a new pattern may emerge. Goldman wisely warns us that it may not be a Western system of institutionalized dissent, but it

still may be very different from China's old forms of ideological consensus. In that ancient country, the modern era is still in its infancy.

MICHAEL GASSTER
Rutgers University

EDWIN HIRSCHMANN. *"White Mutiny": The Ilbert Bill Crisis in India and Genesis of the Indian National Congress*. Columbia, Mo.: South Asia Books. 1980. Pp. 331. \$24.00.

The founding of the Indian National Congress in 1885 has long been considered a turning point in the history of modern India. Discussions of why Indians came together on a nationwide basis to present their grievances at this time usually mention the "demonstration effect" of the 1883 agitation by nonofficial Britishers against the Ilbert Bill. Edwin Hirschmann's minutely detailed account of the 1883 controversy shows why it deserves to be called a formative event.

The Ilbert Bill provoked a confrontation between two communities whose emergence had been cavalierly nurtured by liberal policymakers fifty years previously, when India was opened to large-scale development by nonofficial Britishers and the way was prepared for the opening of the highest government offices to Indians trained in the English language. By 1883 Indians who had qualified for government service had advanced through the ranks to the point where they might logically exercise criminal jurisdiction over the then sizable population of British planters and traders. If the liberal optimists of the 1830s had been right, increasing contact between Indians and Britishers would have bred increasing harmony and mutual respect. Instead, vested interests set them at odds. As English-educated Indians employed the rhetoric of English liberalism to expand their opportunities for participation in British Indian government, nonofficial Britishers, whose survival depended on the perpetuation of race-based privileges, were busy building a profitable sphere for themselves.

The autocratic government of India prided itself on its impartial indifference to partisan pressures. But when the Ilbert Bill threatened to give Indian judges jurisdiction over them, British planters and traders set out to prove that pressure could work in India no less than in Britain. Their noisy success left English-educated Indians furious—and instructed. Surendranath Banerjea advised his fellow countrymen, "You have seen . . . the triumph of a great agitation. I would ask you to imitate the persistence and firmness of the Anglo-Indian agitators, discarding of course their bitterness and violence" (p. 266). The Indian National Congress was launched.

Hirschmann provides a clear, well-told account of the events of 1883. Impressive data show just how

extensive the campaign became within the British Indian community, from Coorg to Assam. Hirschmann makes only a limited effort, however, to place his monograph in a broader historical perspective. Unfortunately, too, the book is poorly produced and marred by a good many printer's errors. Nevertheless, scholars of the period will welcome its reliable marshaling of information.

FRANCIS G. HUTCHINS
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ERLAND JANSSON. *India, Pakistan, or Pakhtunistan: The Nationalist Movements in the North-West Frontier Province, 1937–47*. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 119.) Uppsala: Historiska Institutionen vid Uppsala Universitet; distributed by Almqvist and Wiksell International, Stockholm. 1981. Pp. 283. 106.50 KR.

Historians keep searching for answers to the question, Why was India partitioned? The shape of those answers begins to appear, as evidence from the local Indian political scene in the 1920s, 1930s, and 1940s comes to light. In any assessment about the partition of India, the politics of the North-West Frontier Province (NWFP), which is now a part of Pakistan, must figure prominently. The NWFP entered modern Indian political life after World War I—a late comer to the already vigorous nationalist movement in the rest of the country. The province borders Afghanistan and its preponderant ethnic group in the 1940s was Pakhtun (Pathan or Afghan). In addition, over 90 percent of its population was Muslim, thus making it the most heavily Muslim province in India. Low literacy, an absence of industry, and the British desire to keep the Pakhtun tribal areas as a semiautonomous border zone resulted in political backwardness as well as maintaining a semifeudal economic structure, which flourished in the settled districts. Then, in the 1930s a mass movement, which called itself the Khudai Khidmatgars ("Servants of God"), arose under the leadership of the brothers Abdul Gaffar Khan and Dr. Khan Sahib. Organizing peasants and small landowners more effectively than perhaps any other political movement in the subcontinent, it stood for social and economic reform, Indian independence, Islamic values, and autonomy for the Pakhtuns. Allied with the Indian National Congress, the Khudai Khidmatgars, whom the British called "Red Shirts," became the most powerful demonstration that Indian nationalism was noncommunal, that is, that it overshadowed Hindu-Muslim differences. For that very reason the Muslim League under the leadership of Mohammad Ali Jinnah collaborated with the big landowners in order to oppose the Khan brothers and their Congress ministry. In the 1930s Jinnah and his allies tried to win over the

NWFP to the cause of Muslim nationalism and in 1946–47 to the cause of Pakistan independence. If the Congress could hold the NWFP despite Jinnah's claim to represent Indian Muslims, the Pakistan movement would fail.

Erland Jansson's valuable book explains how a combination of local and national politics eventually threw the NWFP into the camp of the Muslim League and Pakistan. Local Muslim officials, students, and religious leaders eroded the Congress's following with appeals to Islam in danger. But possibly more critical, the Congress under Jawaharlal Nehru gave in, against the wishes of Mahatma Gandhi and Abdul Gaffar Khan, to the partition of India, thus leaving the NWFP hanging on the far side of emerging Pakistan with its Congress ministry clinging to the lost ideal of a united India and an autonomous Pakhtunistan. One month before the British left India a referendum was held in the NWFP, and the pro-Pakistan voters won overwhelmingly, partly because the Congress boycotted it. That was the end of all-Indian nationalism on the frontier.

But integrating the NWFP into Pakistan did not effectively follow. Today local issues with which Jansson's book deals historically—for example, landlordism, economic backwardness, and Pakhtun identity—still have not been dealt with by a military regime whose appeal, as was Jinnah's in the 1940s, is based on the threat of Hindu (Indian) domination.

In addition to his scholarly tracing of NWFP politics, which is based on all available archival evidence in Pakistan, India, and London, Jansson offers a wise judgment on modern nationalism—that it is often reduced to local issues and personal relationships. He contradicts the long-standing dictum of many Indian historians that the British and Sir Olaf Caroe, the governor of the NWFP, had a determining role in the creation of Pakistan. The evidence continues to mount that while the British ruled, the Indians divided.

CHARLES H. HEIMSATH
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THOMAS HODGKIN. *Vietnam: The Revolutionary Path*. New York: St. Martin's Press. 1981. Pp. x, 433. \$30.00.

Thomas Hodgkin is an Emeritus Fellow at Balliol College, Oxford, and a specialist in African affairs. In 1974 he spent three months as a guest of the Institute of History in Hanoi, during which time he became fascinated with the sources and special characteristics of Vietnamese revolutionary tradition. The result of that interest is this book.

At first glance, the title, *Vietnam: The Revolutionary Path*, appears somewhat misleading. The reader expects a book that begins with the rise of the revolutionary movement during the colonial era and climaxes with the fall of Saigon in the spring of 1975. In actuality, nearly half of the volume deals with the precolonial period, and it concludes with the launching of the August Revolution at the end of the Pacific war in 1945. It is a salutary choice. Where earlier studies of the Vietnamese revolution have commonly sought the origins of Communist success in the colonial era, Hodgkin attempts to trace its roots back through several thousand years of history. By concluding with the August Revolution, the author suggests that with its seizure of power in Hanoi in 1945, the Communist-led Viet-minh movement had already laid legitimate claim to the mantle of leadership in the struggle for national liberation. Only foreign intervention—first by the French, and later by the United States—prevented the Communists from harvesting the full fruit of their labors until three decades later.

The major theme in the book centers around the historical struggle of the Vietnamese people to preserve their national independence, first against the smothering embrace of great neighbor China, more recently against the imposition of French colonial rule. It reaches a climax with an exhaustive treatment of the emergence of the Communist movement under Ho Chi Minh as the leading force in the struggle for independence and social justice. Hodgkin's treatment is frankly sympathetic, not only to the Vietnamese people, but also to the overall cause of social revolution in the developing world. Indeed, he suggests that an alternative title for the volume might be "The Intelligent Young Third-World Radical's Guide to Vietnamese History."

If there is a problem here, it lies mainly in a certain confusion over the prospective readership. The book is quite detailed and may tax the patience of any but the most persistent general reader. On the other hand, Hodgkin is not an Asian specialist, and, although he offers some interesting interpretations on several historical issues, he does not probe with the skill of the expert for the historical links between Vietnamese traditional culture and the contemporary force of social revolution. The specialist searching here for answers to such questions may be somewhat disappointed and will find the book most useful as a handy reference work for information on the traditional period and the rise of the Communist movement.

Aside from its minor analytical weaknesses, the book has much to offer anyone with the patience to see it through. Hodgkin is to be applauded for providing the specialist and the generalist alike with

a provocative and well-documented study of one of the most significant social upheavals of modern times.

WILLIAM J. DUIKER
Pennsylvania State University

ROBERT HEUSSLER. *British Rule in Malaya: The Malayan Civil Service and Its Predecessors, 1867–1942*. (Contributions in Comparative Colonial Studies, number 6.) Westport, Conn.: Greenwood Press. 1981. Pp. xx, 356. \$37.50.

JAGJIT SINGH SIDHU. *Administration in the Federated Malay States, 1896–1920*. (East Asian Historical Monographs.) New York: Oxford University Press. 1980. Pp. xvii, 227. \$24.95.

Both of these monographs represent a return to the preoccupation historians once had with the administrative history of Malaya under British rule, but, whereas the book by Jagjit Singh Sidhu is very much in the old mold, Robert Heussler's provides a fresh approach to the subject.

Sidhu sets out to fill what he sees as a gap in the history of Malaya by concentrating on the administration of the Federated Malay States from 1896 to 1920. He traces the process by which first the resident-general and the federal bureaucracy centralized power in Kuala Lumpur and then the governor in Singapore began to exert greater control from outside the federation, and he presents a fine analysis of the contradictions inherent in the creation of the federal council. In a chapter concerned with such matters as Malay education, the Malay Reservations Enactment, and the creation of the Malay Administrative Service, he examines the "disastrous" effects of British policies on the Malays. A chapter on "The Diminution of the Malay Elite" argues that the British weakened, even humiliated, the sultans by, among other things, altering the rules of succession to suit their purposes. One might suggest, however, that the Malay rulers were not always passive recipients of change, as Sidhu's own account of Sultan Idris's attempts to have his son succeed him illustrates very nicely. In the same chapter Sidhu claims that the sultans' authority was undermined as "the Malay masses grew more aware of their sovereigns' powerlessness" (p. 126). Valid though this idea might be, he presents no evidence to support it.

As for the book as a whole, it is a useful survey of British administration during a very important period. Nevertheless, much of what he has to say will already be familiar to those who have read the studies by Emily Sadka, Eunice Thio, Rex Stevenson, and others on aspects of this period. Perhaps of more importance, Sidhu tackles his subject very

much in the traditional manner. At the very least, there is a need to adopt a broader conception of "administration." One might note, for example, that in the early 1900s the government moved to tighten its control over the Chinese population and to abandon the system of farming out the collection of certain taxes to leading Chinese businessmen. The changes experienced by the Malay rulers were part of a larger, as yet unexamined, process of change.

Heussler deals with most of the same old topics covered by Sidhu and many others, since he looks at British rule in all of "British Malaya" from the time of "intervention" to the Japanese occupation. His main concern, however, is not policy but the civil servants who made it and tried to carry it out. "Unlike civil servants in England," he observes, "they were a ruling rather than a bureaucratic group" (p. 326). Heussler discusses in vivid detail the changes in recruitment, training, daily routine, and career patterns that took place over the three periods he examines: the pioneering phase of the first residents, "the middle years" from federation to World War I ("the most uncomfortable and the most productive" of the three periods), and the interwar years.

The great strength of his book is his insight into personal relationships within the civil service and between the civil service and officials in the Colonial Office. Throughout the study he drives home the point which he makes in his account of the decentralization controversy that "issues and personalities were hard to separate" (p. 252). Heussler is a master of the skillful turn of phrase and the deft characterization. "His strong point was affability," he writes of E. W. Birch (p. 36). Limitation of space prevents a listing of even the major topics in Heussler's book, but reference to his chapters on the Chinese Protectorate, the Labor Department, British advisers in the unfederated states, and the work of residents and district officers gives some indication of the study's breadth. His effective use of a great variety of sources, especially the interviews he conducted with retired civil servants, gives this book enduring value.

In one respect, however, Heussler's handling of his sources raises a problem. One is struck by the frequency with which he adopts the perspective of the men he is writing about: "there was always a tendency for some Malay officials to backslide" (p. 297); British officers had "the never ending job of trying to protect the Malays against themselves" (p. 298); and when the sultan of Trengganu abdicated the British adviser "was at last in a position to move forward" (p. 213). Although the reader can compensate for such statements, they do tend to obscure the views Malays must have had about the profound changes they were experiencing.

A further comment on Heussler's book may be in order. The book appears in a series entitled "Contributions in Comparative Colonial Studies." In the foreword the series editor comments that in the case of *British Rule in Malaya* the element of comparison is explicit for anyone who is familiar with Heussler's extensive writings on British administration in Africa. Unfortunately, this element is not obvious to the reader who lacks this advantage. One can only hope that Heussler will indeed undertake a comparative study of British rule in Africa and Malaya. No one is in a better position to do so.

JOHN G. BUTCHER
Griffith University

A. W. MARTIN. *Henry Parkes: A Biography*. Carlton, Australia: Melbourne University Press; distributed by ISBS, Forest Grove, Oreg. 1980. Pp. xiii, 482. \$44.00.

From the 1850s until the early 1890s—that is, from the era of the coming of responsible government down to the decade that culminated in Confederation—Henry Parkes was the dominant figure in the political life of New South Wales. His papers in Sydney's Mitchell Library are the single largest Australian collection for the nineteenth century. Allan W. Martin has worked on this book for many years. Indeed, his previous research on Parliament factions and parties, published with Peter Loveday in 1966, was, in part, preparation for this biography. The challenge was therefore considerable. The payoff on a "life and times" of Parkes should be extremely rich.

In his preface Martin disarmingly confesses that he is not entirely happy with his achievement. He thought long and seriously about a thematically organized book. Instead, his presentation is strictly chronological. Martin does not explain why he abandoned the thematic approach, but one can guess. Such a work might be psychohistorical. Martin does have a good deal to say about the private person—Parkes published several volumes of bad poetry, had a mid-life identity crisis, and married very young women when in his seventies—but there is far too much public material in the record that is not readily susceptible to psychological analysis and that would have had to have been discarded. A thematic study might be organized by issues and great events. (That, for example, is how, within a very loose chronological framework, Sir Keith Hancock organized his *Smuts*.) The trouble is that the internal political discourse of New South Wales, which after all *was* Parkes's life, was not organized that way. There were issues, of course—the drive of the young society for autonomy from Britain; the

Irish question, centering on control of public education; the tariff; Confederation—and Martin makes good use of them. But they were perennial problems, only rarely coming to a climax. Parkes, moreover, was not an issue-oriented politician: for him the game of winning and holding power was an end in itself. Writing narrative political history is Martin's game, too.

Martin tells his story in strictly chronological chapters that bite off relatively short chunks of time. Some of them, it must be said, are barely digestible. The reader is bombarded with too many names, too many quotations from newspapers or letters, too many unmemorable incidents, too many movements in or out of office. That, again, is the way it was. Nineteenth-century Australia had factions but not parties. Public discourse was provincial and parochial, and statesmen did not arise until Confederation. If Martin was going to tell it like it was—that is, as Parkes lived it—within a book of less than five-hundred pages, then it is hard to imagine a book less dense and less complex.

But, like Martin himself, I wonder. Could he not have used a looser chronological framework? Could he not have written the book in parts, covering fifteen-to-twenty year intervals, with chapters on particular themes? One of those themes might well have been the changing nature of public discourse in New South Wales. I can conceive of several chapters, one in each part, which would present the main facts of Parkes's political career, but which would be devoted primarily to illustrating Martin's own generalizations about Australian politics and society in the last half of the nineteenth century.

I finished the book a little disappointed, having learned a good deal about Parkes, but rather less than I had hoped about Australia. There are some important insights. Parkes's career, for instance, beautifully illustrates the "fragment" thesis of Louis Hartz's *Founding of New Societies*: the young radical from Birmingham, in the Thomas Attwood mold, fought in Australia for the Reform Bill; having achieved it, in the shape of "Responsible Government," he was basically satisfied. By the early fifties, when he visited England, he was already out of touch with a "liberalized" society that had left behind not only the Reform Bill but also Chartism. In England Parkes was an anachronistic radical; in Australia he was a conservative, at times fanatically anti-Irish and with no great sympathy for the Sydney working class. But these important insights about Australian society are comparatively rare. Martin's very tight chronological framework has not prevented him from writing a good book. It has, I believe, barred the way to a great one.

JOHN W. CELL
Duke University

JOHN ROBERTSON. *Australia at War, 1939–1945*. Melbourne: William Heinemann. 1981. Pp. viii, 269. A \$27.50.

I. C. MCGIBBON. *Blue-Water Rationale: The Naval Defence of New Zealand, 1914–1942*. Wellington: New Zealand Government Printing Office. 1981. Pp. xx, 446. NZ \$45.00.

Australia and New Zealand prepared for and fought strangely different wars in 1939–45. It is thus appropriate that these two studies of the military experience of the British Dominions in the South Pacific should represent well-nigh totally different approaches to the writing of military history. I. C. McGibbon's book is a classical official history, written in the unfailingly lucid but relentlessly non-literary style of a New Zealand government report, undeviatingly logical in its organization and meticulously documented, apart from one or two extraordinary omissions. John Robertson, by contrast, has adopted an approach uncomfortably combining the thematic and the chronological, presented in an animated and attractive style that occasionally splutters surprisingly into outbursts of moral indignation over the iniquities of sly entrepreneurs, arrogant and conceited public servants, and cowards skulking in the grass. Apart from that, McGibbon has written what is close to being the first and may very well be the last word on the subject. Robertson has made an uncertain contribution to a field to which further contributions were perhaps not too urgently required.

McGibbon's basic problem is, of course, that it is difficult to produce a significant study of the naval defense of a country that has never been in any real danger of being either invaded or blockaded and that could never make any effective contribution to its own maritime security against either threat. Nor could one write a full-length history of a navy that had an effective strength of two light cruisers and an armed merchant ship. McGibbon has perforce had to write a study of British plans for the naval defense of the eastern empire as they affected New Zealand. One might well feel that he has exhausted the subject. The almost total omission of the Australian angle is, of course, quite appropriate. It would, however, have been enlightening if McGibbon had made more specific the fact that the New Zealanders were only spasmodically interested in developing any cooperative defense plans with Australia because they were in a totally different strategic position. Australia not only had far greater reason to regard itself as a target for invasion, but also had far greater capacity and consequently far greater responsibility to make some significant effort in its own defense. The New Zealanders could hardly be

said to have a defense problem because they could not defend themselves at sea. They could only count upon the United Kingdom. The Australians did, however, have enormous problems as to exactly where their own defense effort could most rationally be directed. Debate on the nuances of regional defense in Canberra was frequently conducted with considerable intellectual vigor and, as Carolyn A. O'Brien has shown, unexpectedly Jamesian refinement. McGibbon could helpfully have suggested his readers to turn to O'Brien's analyses for a wider view of Pacific and Indian Ocean strategy in the years between the wars. It is also extraordinary that he should have omitted any reference to the two joint Australian–New Zealand secret missions to Washington in November 1940 and May 1941, documentation for which was found by the reviewer in the National Archives of New Zealand.

McGibbon may nonetheless have filled a real gap; Robertson has hardly done this. There is no lack of both scholarly and popular books and articles on Australia's role in World War II. An informative and readable summary is, however, always useful, and this is what Robertson's book succeeds in being. It does not, however, fill this niche too comfortably. Robertson's documentation is useful but certainly not comprehensive and cannot substitute for the very full, very numerous, and often very good official histories and scholarly studies. Nor does his confusing organization of chapters make for an easy read for the nonscholarly public. He is, however, extremely strong on nuts-and-bolts data and has many pertinent and lively comments to make regarding the conspicuous failings of some of those who held positions of responsibility during the most critical period of Australian history. His specific contribution may be to provide a quick fix for high school or university teachers preparing lectures on Australia in the war of 1939–45. They are likely to find in this relatively brief volume almost everything they might need to know in a hurry.

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UNITED STATES

RICHARD J. HOOKER. *Food and Drink in America: A History*. Indianapolis: Bobbs-Merrill. Pp. xii, 436. \$14.95.

This book, in spite of its notes and the academic credentials of its author, is not a scholarly history of food and drink in America. Rather, it is a fact-filled compendium of our nation's changing and change-

less dietary habits. Here we learn these tidbits: refugees from the French Revolution brought the word restaurant to the United States; as early as 1802 the tomato was introduced and rejected; in 1858 at the inauguration of the governor of the Nebraska Territory coffee was passed around in a washtub; not until 1868 was it respectable for women to eat lunch unescorted in restaurants; the 1890s brought Campbell's canned soup, Jell-O, and Van Camp's beans in tomato sauce, while the 1920s gave us Howard Johnson's orange-roofed restaurants and Clarence Birdseye's frozen vegetables; and by 1980 one-third of all American meals were eaten out, often in fast food chains.

This sampling both gives the flavor of Richard J. Hooker's book and reveals its two great deficiencies. One deficiency is the lack of a theme. Only in the last chapter does Hooker depart from his list of anecdotes to lament recent trends, such as the contradiction between clean, efficient kitchens and tasteless, chemically laced food prepared within them. These points might have been the beginning of a provocative book on the recent American diet, but that is not the book Hooker wrote. The second deficiency is the lack of method. Using the techniques of social history common one-half century ago, and relying heavily on travel accounts, the author often confuses the eating and drinking of his socially upwardly biased sources for the dietary habits of the average man. He all but ignores scientific studies of diet, and he never establishes in any systematic way a precise description of the changing content of what Americans ate or drank. I would not expect a popular history to be filled with charts, tables, and calculations of caloric intake, but the failure either to undertake such research or to incorporate that kind of research by others leaves the work as a mélange of generalities decorated with glittering anecdotes.

Hooker's book recalls older social histories such as Alice Morse Earle's *Stage-Coach and Tavern Days* (1900) and Guion Griffis Johnson's *Ante-Bellum North Carolina* (1937) or recent popular accounts such as Stanley Baron's *Brewed in America* (1962) and J. C. Furnas's *The Americans: A Social History* (1969). (Indeed, Hooker cites these titles eleven, six, seven, and eight times respectively.) One must admire the author's devotion to his topic, his labor, and particularly his ferreting out of numerous travel account stories concerning the American diet. Such labors have led to a book that serves admirably as a reference work, a task facilitated by an excellent subject index. Then, too, the notes, while they include only four books published since 1974, are a competent guide to the subject and are particularly important for giving access to dozens of unindexed travel accounts. Finally, despite its limitations, the

reader will find *Food and Drink in America* informative and entertaining.

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E. P. HUTCHINSON. *Legislative History of American Immigration Policy, 1798–1965*. Philadelphia: University of Pennsylvania Press, for the Balch Institute for Ethnic Studies, Philadelphia. 1981. Pp. xv, 685.

As the title indicates, this work is a legislative history of American immigration policy as revealed by the actions of Congress during the period 1798–1965. It traces "one of America's largest and most complex bodies of legislation perhaps exceeded only by the federal tax code." In the process of legislating, the Congress gradually worked out what eventually amounted to a national policy concerning immigration into the United States.

The book is divided into two parts. Part 1 gives a chronological account, session by session, of congressional action on immigration bills whether eventually enacted or not. E. P. Hutchinson traces the congressional history from the legislation associated with the anti-immigrant fears of the Federalist party during the John Adams administration through the ensuing presidential administrations, as humanitarian concern over the conditions of immigrants aboard vessels bound for the United States, fears that this country would become the dumping ground for Europe's paupers, and the beginnings of anti-Chinese feeling in the Far West were reflected in the presentation of bills of one sort or another to meet these particular problems. Slowly and reluctantly, the Congress finally accepted the role of assuming federal control of immigration. The rest of part 1 deals with the gradual widening of that control as a result of anti-Chinese and anti-immigrant labor feeling. The Congress's gradual move from mere regulation to outright restriction of immigration follows, eventually ending with the passage of the noted "quota" legislation of the 1920s. Part 1 is completed with the actions taken by the Congress in the post-World War II period, which eliminated the racial and ethnic features of the immigration policy of the 1920s. At the end of each chapter, the author adds a valuable comment and summary explaining how the congressional actions fitted into the prevailing American feeling of the moment.

Part 2 examines the various immigration acts passed by the Congress, outlining the lines of continuity that run through this body of legislation. An analysis of the immigration actions of the colonies and states before federal control is followed by

chapters dealing with the various features of federal policy: selection by exclusion (criminals, public charges, mental defectives, so-called immoral classes, and other undesirables), selection by deportation (subversives, illegal immigrants, criminals, and public charges), restriction of immigration (through the head-tax, literacy test, quota system, and suspension of immigration), and maintaining racial and ethnic composition (through Oriental exclusion, literacy tests, and the quota formulas). An examination of other aspects of immigration policy follows (labor market policy, family unification, refugee asylum, and the technical aspects of carrying out immigration policy). Hutchinson concludes with a consideration of some remaining issues facing any future immigration policy. Again each chapter ends with his summary and comment.

The book is well documented and annotated. It should prove to be a valuable reference source for every research, legal, and reference library.

EDWARD G. HARTMANN
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STEVEN KESSELMAN. *The Modernization of American Reform: Structures and Perceptions*. (Modern American History.) New York: Garland. 1979. Pp. 645. \$40.00.

Two questions address the evolution of ideas from the Gilded Age to the New Deal: why a set of ideas, available at one time, does not become a "coercive truth" for society until another period; and why, of all the intellectual alternatives available at any one time, one configuration of ideas generates a popular response. Both questions are intelligently handled in Steven Kesselman's study of six reform intellectuals and their reactions to industrial society.

Kesselman divides his writers into three pairs. The first pair, Henry George and Edward Bellamy, employed a "monistic" structure (rationalistic, absolutistic, romantic) to maintain traditional values and preserve natural law; they went beyond complexity and dynamism to seek simple and universal principles during an age of uncertainty and chaos. A second set of thinkers, appearing at the turn of the century and characterized by the writings of Jane Addams and Herbert Croly, represented "a halfway stage between reform as education of the public to engage in collective action, and reform as persuasion of the public to delegate its powers" (p. 194). Reacting to a society in transition, they precariously balanced dualisms such as "creativity and adaptation" and "authenticity and objectivity." The New Deal professionals, Rexford Tugwell and Thurman Arnold, the third pair, accepted the view of industrialization's maturation and adopted a "pluralistic"

structure calling for planning and random experimentation. Kesselman concludes his narrative with a discussion of the "postmodernization of American reform," beginning in the 1960s and, ironically, possesses structures and perceptions similar to those of Bellamy and George—written one hundred years earlier.

Kesselman carefully analyzes their writings—in particular, George's small enterpriser, Addams's pacifism, and Arnold's legalism—and why some of their books had more appeal than others. He is at his best in showing how their ideas were shaped and influenced by such notable thinkers as William G. Sumner, Roscoe Pound, Simon Patten, and others. And he has done more than any previous historian in examining the changing perceptions of social thinkers who responded to the problem of modern industrialization.

Weaknesses, however, do appear. Kesselman's study, apart from its selectivity, suffers from repetition and overuse of such words as "generalizability" and "completable." He also loses his focus with an excessive exposition of social theorists responsible for formulating the intellectual structures of his main figures—for instance, in one chapter almost ten pages are devoted to Pound's juridical views. He could have economized on their impact without losing consciousness of his own reformers. Structurally, a major problem exists in his failure to place his thinkers in a wider social context. In many cases their views are discussed in a vacuum, with little reference to actual events, thereby creating the impression that they are ineffectual and bankrupt.

Nevertheless, this book should be read by intellectual historians interested in examining the polarity of thought of a select group of liberal thinkers whose reforms were geared to an ideal order rather than to the perceived one. It is a unique approach that specialists, as opposed to a general readership, can appreciate.

CHARLES F. HOWLETT
Columbia University

A. G. ROEBER. *Faithful Magistrates and Republican Lawyers: Creators of Virginia Legal Culture, 1680–1810*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1981. Pp. xix, 292. \$24.00.

Virginia's county court system provides A. G. Roeber with ample material for analyzing the impact of "country" and "court" ideology on law and politics in the Old Dominion from the late seventeenth century to Jeffersonian times. Quoting J. G. A. Pocock, he observes that Virginia was particularly "country minded" in its attitudes, and that its

county courts exceeded their English counterparts in both jurisdiction and prestige (pp. xviii, 44). With the rise of trade, however, this simplistic and non-professional approach to law began to give way, albeit reluctantly, to the needs of litigants and the public for a more efficient system of justice. In a semiliterate society the rituals of court day were vital in reaffirming law and social relationships; as literacy increased and the bar became sophisticated these symbolic functions became less important, and by 1735 critical comments appeared in the public press concerning the quality of justice in the local courts.

The county court act of 1748, separating general court lawyers from practice before local courts, led to an accelerated decline of the county courts (pp. 107–12). Neglect of duty through absenteeism and class favoritism fed the fire of public criticism. Even the American Revolution's infusion of new families into the gentry pool did not stay the rapid decline of the county court system. The years from 1780 through 1800 were marked by gradual paralysis of the county courts, accompanied by reforms (such as the institution of district courts in 1787) that were designed to apply general court practice standards to county-level trials.

As Roeber ably demonstrates, the tension between trained lawyers, admitted and regulated by the governor, and the local judiciary, supported by their independent wealth and antagonistic to "court" influences, was a constant factor throughout the colonial and early national period. Eventually it split the Virginia Republican party into a moderate wing, led by Jefferson and supportive of an educated bar, and a radical wing, led in part by John Randolph of Roanoke and vehemently in favor of the "country" ideology.

This thoughtful and well-researched monograph provides some interesting parallels to recent scholarship on other early American legal systems. For example, Roeber's recognition of the county court's declining concern with moral offenses and increased attention to debt collection (pp. 187, 216) bears comparison to William E. Nelson's similar conclusions for Massachusetts in the same period (*Americanization of the Common Law*). On the other hand the reach of Roeber's imagination may have exceeded his grasp of the facts, for it is difficult to accept the opinion that the Virginia experience with county courts and its commitment to "country" ideology shaped Southern attitudes toward law and court organization (p. 258). While there is no doubt that the Virginia county court system profoundly influenced Maryland, parts of North Carolina, and all of Kentucky and Tennessee, it is extraordinarily difficult to force South Carolina into the "county court" mold. Indeed an abortive experiment with county courts between 1785 and 1800 convinced South Carolinians that their judicial system could survive

without such local tribunals and should suggest to thoughtful historians that Virginia's county court system may well have been based on historical and geographical factors, as well as upon the prevalent "country" ideology of the Virginia gentlemen.

HERBERT A. JOHNSON

University of South Carolina

JOSEPH A. CONFORTI. *Samuel Hopkins and the New Divinity Movement: Calvinism, the Congregational Ministry, and Reform in New England between the Great Awakenings*. Grand Rapids, Mich.: Christian University Press. 1981. Pp. viii, 241. \$16.95.

This book is an impressive contribution to a growing body of scholarship that revises our understanding of eighteenth-century American religious history. Until the publication of Alan Heimert's controversial epic, *Religion and the American Mind*, Samuel Hopkins and other proponents of the New Divinity were regarded as excessively zealous disciples of Jonathan Edwards, Puritans gone haywire, who transformed the rich paradoxes of reformed Christianity into simple-minded abstractions. The career of Samuel Hopkins served as a perfect illustration of the rigorous but ludicrous version of Calvinism that swept through western New England after the Great Awakening and carried some of the brightest ministers out of the mainstream of American history and into oblivion.

After graduating from Yale in 1741, Hopkins studied with Edwards at Northampton, then took his evangelical views to Great Barrington, a frontier village that for twenty-five years resisted Hopkins's notion that most of the inhabitants were vile sinners whose efforts at leading holy lives actually offended God; eventually they dismissed him. He ended up in Newport, a city that symbolized all the luxury and venality that Hopkins detested. Although he was an ardent supporter of independence from England and an early critic of slavery and the slave trade, most historians have seen him as a failure, both as a minister and as a theologian whose insistence on an extreme version of divine sovereignty and human depravity flew in the face of the emerging liberal ideals of the Enlightenment.

Joseph A. Conforti tells a different story. In his view, Hopkins and the New Divinity ministers were authentic spokesmen for "the first indigenous American school of Calvinism" (p. 3). He admires the sheer rigor and comprehensiveness of Hopkins's theology and, like the forthcoming book by Bruce Kuklick, asks us to take the New Divinity seriously as a philosophical movement that deserves an important place in the history of ideas in America. By holding tenaciously to his views rather than telling his parishioners what they wanted to hear,

Hopkins preserved the intellectual integrity of his theology. By choosing alienation over accommodation, he and the other New Divinity thinkers became the first "lost generation" in America.

But Conforti's major contribution is to establish a clear connection between the central ideas of Hopkins and his New Divinity colleagues and the social history of eighteenth-century New England. In a splendid chapter on Hopkins's doctrine of "disinterested benevolence," Conforti argues that Hopkins's theology was a coherent protest against the evolution of New England society. His apparently perverse insistence on self-denial was a plausible response to the individualistic ethic that accompanied the rise of capitalism. In Conforti's hands the New Divinity theology becomes the ideology of the small and isolated villages of western New England, where the traditional values of community lingered on well into the nineteenth century and where, in fact, most New Divinity ministers lived and worked. The frustrations Hopkins experienced in Newport—Conforti entitles this chapter "A Puritan in Babylon"—take on the character of a noble defense of communal values against the cupidity and narcissism of the marketplace.

Although those predisposed toward Reaganomics will find this interpretation objectionable, Conforti has grounded his argument in the original sources. And his endnotes display a thorough familiarity with the secondary literature generated by traditional students of religious history as well as social historians. Although it began as a doctoral dissertation at Brown, the shuffle of note cards is not audible in the background and the prose is lucid and lean, if not elegant. Not only does the book immediately become the standard life of Samuel Hopkins, but it also vitalizes the scholarly debate over the significance of the New Divinity and contributes to the synthesis of "the new social history" and the old-fashioned interest in American thought.

JOSEPH J. ELLIS
Mount Holyoke College

CHARLES ROYSTER. *Light-Horse Harry Lee and the Legacy of the American Revolution*. New York: Alfred A. Knopf. 1981. Pp. xiii, 301. \$15.00.

"This book is a study of the effect of the American Revolutionary War on the character and career of Henry Lee" (p. xi). So begins Charles Royster's compelling account of one of the most fascinating of the Virginia Lees. Royster offers neither full biography nor a life-and-times study of the Revolutionary era. Rather he writes selectively, focusing attention on "the connection between character and war" (p. xii). His purpose is "to trace the legacy of the war in Lee's life and to analyze the ways in which the

American Revolution dominated the period that came after it" (p. xi).

Chapter 1 deals with the war itself and Light-Horse Harry Lee's familiar role as leader of the famed Southern partisan brigade. These were the years, Royster argues, that gave lasting definition to Lee's life. From his brief yet intense war experience, Lee developed a profound identification with the republican cause, a close equation of his own shifting circumstances and the nation's destiny, and a vision of a peacetime America free from the disorders of war, prosperous, orderly, and secure under disciplined leaders such as himself.

In the remaining six chapters of the book, Royster traces the parallel collapse of Lee's personal vision, his disillusionment with what he perceived to be the increasing disorder within the republic, and the deterioration of his personal circumstances. Lee's postrevolutionary experience is a sorry story of disastrous land speculation, failed commercial ventures, strained family relations, and political isolation. He spent a period in debtor's prison, was nearly killed by an anti-Federalist mob, and sought shelter in the West Indies before returning to Georgia where he died, alone and unmourned, in 1818.

The story is well told. Royster has a fine dramatic sense, and he draws upon it effectively to illuminate analysis and discussion. Lee emerges as a fully credible individual, alive and capable of attracting interest and sympathy, in spite of his monumental flaws—or perhaps because of them!

And Royster is surely correct in his larger assertion that the revolutionary experience left its mark on Lee's generation. The war and all that surrounded it provided for its participants a compelling frame of reference, a set of standards by which to judge their own and future generations' behavior. The argument is not new, nor is its corollary that postrevolutionary experience, measured against the memories of selfless and disciplined revolutionary enthusiasm, proved disillusioning to many. Yet Royster's sensitive and richly textured evocation of one revealing life, properly qualified by warnings of the exaggerated and idiosyncratic character of Lee's behavior, deepens our understanding of the ways in which experience, mythologized by memory, shaped the lives of the Revolutionary generation.

The argument is not without its problems, however. One comes away from the book still wondering how much of Lee's later disillusionment is in fact traceable to his earlier revolutionary commitments, and how much to his own distinctive personality? Did the war experience turn Lee's development in new directions, or did it reinforce character traits already firmly in place? It is not altogether clear whether Royster firmly establishes the connection between Lee's war experience and his postwar be-

havior, or simply asserts it. In much of what Royster has to say there is a disturbing determinism between earlier experience and later behavior, yet it is not fully examined. Indeed, the psychological bases of Royster's interpretations are never directly addressed, though the analysis is filled with assumptions about Lee's behavior. Nor does saying that Lee's personal bankruptcy "mirrored the dismantling" of his national vision really explain the connections between the two.

But perhaps that is to demand too much. Perhaps we should be content to admire the book for what it is—a provocative, revealing, and speculative portrayal of a fascinating figure out of our Revolutionary past.

JOHN HOWE
*University of Minnesota,
Twin Cities*

SUZANNE GEISSLER. *Jonathan Edwards to Aaron Burr, Jr.: From the Great Awakening to Democratic Politics*. (Studies in American Religion, number 1. (Lewis-ton, N.Y.: Edwin Mellen Press. 1981. Pp. ix, 276. \$24.95.

Suzanne Geissler's new study of Aaron Burr promises much. Put most briefly, she argues that Burr is best understood as a religious descendant of his Calvinist–New Light grandfather, Jonathan Edwards, and father, Aaron Burr, Sr., and that Alan Heimert's controversial thesis in *Religion and the American Mind* (1966) "is essentially correct and the experiences of the Burr family substantiate that conclusion" (p. 2). Unfortunately, Geissler has not proven Heimert's thesis and in fact has committed the same serious error that characterized Heimert's study.

Geissler begins with a brief historiographical summary, criticizing all previous biographers for "ignoring Burr's evangelical background as a factor in his political career" (p. 4). She next sketches the career of Aaron Burr, Sr., as a "spiritual son" of Jonathan Edwards, supporter of the Great Awakening, and president of the College of New Jersey. The majority of the book then traces the career of Burr, Jr., emphasizing his "Edwardsean implanting" and alleged evangelical aspects of Burr's military and political exploits. The author concludes that Burr's political ideology was "in the Edwardsean tradition" (p. 238) and that he represented a "logical (and not irreligious) conclusion of Edwardsean post-millennialism [*sic*]" (p. 239).

In order to support this weighty conclusion the author is frequently reduced to speculation. The book is flawed because Geissler, in order to connect Burr to the Edwardsean tradition, often simply asserts that connection without supporting evidence. Space permits only the citation of three

examples. Geissler notes Burr's 1773 statement that "the road to Heaven was open to all alike" (a statement all earlier biographers took to mean that Burr rejected the Calvinism of his father and grandfather) and observes that "we do not know precisely what Burr meant by this statement, but we would suggest that taken by itself it is no sign of rejection of Calvinism" (p. 133). Perhaps, but without supporting evidence Geissler's interpretation is no more valid than those she rejects. Likewise, no documentation is provided to support the assertion that Burr's role in the 1775 invasion of Canada made him "an inter-colonial hero, to the overwhelming delight of Presbyterians and evangelicals" (p. 144). Finally, Geissler makes a large point that Burr's 1805 farewell address to the Senate and Jonathan Edwards's 1750 farewell sermon to his Northampton congregation bear "a striking resemblance," even though she admits "there is no evidence that Burr did model his own farewell speech on his grandfather's" (pp. 214–15).

All of these conclusions may be true. The point is that Geissler has not proven them, any more than Heimert proved his thesis linking the Great Awakening to Jeffersonian and Jacksonian democracy. This book, while well written, is an unfortunate example of history by assertion. The earlier chapters on Burr, Sr., are solid and represent the book's strongest contribution. But when Geissler gets to Burr, Jr., the paucity of evidence does not deter her from locating Burr within the evangelical tradition and portraying him as a logical extension of Edwards and the Great Awakening.

In a foreword to Geissler's book Ralph Ketcham observed that "scholars will doubtless challenge these assessments of Burr's career and of the link between religion and politics" that Geissler provides. This reviewer certainly does.

JOHN F. BERENS
Northern Michigan University

DONALD JACKSON. *Thomas Jefferson and the Stony Mountains: Exploring the West from Monticello*. Urbana: University of Illinois Press. 1981. Pp. xii, 339. \$19.95.

It is entirely appropriate that the distinguished editor of the Lewis and Clark, Zebulon Pike, George Washington, and John C. Fremont papers undertake this detailed assessment of Thomas Jefferson's interest in the American West. Although the subject is not a new one, Donald Jackson brings to it a wealth of fascinatingly detailed information conveniently, even entertainingly, summarized between two covers. In what is basically a series of connected essays, the author touches on nearly all of Jefferson's well-known transactions with the West, including the Louisiana Purchase, the West Florida ques-

tion, the Ledyard and Michaux expeditions, the Burr conspiracy, the Red River expeditions, the Astoria adventure, and the launching of Lewis and Clark.

Thanks to the publication over the last fifteen years of an ever growing number of books and studies concerned with the relation of science to the perception of the West, Jackson has also been persuaded to highlight this aspect of Jefferson's Western concerns. He does this very well in discussions ranging from cartography to natural history to the president's fascination with scientific instruments. This provides an insight into Jefferson's thought processes that is often overlooked by political historians. In many ways Jefferson's many-faceted concerns with the West provide a better view of his character than do studies confined solely to his diplomacy, or his presidency, or for that matter his attitude and behavior toward the Negro. For Jefferson the West was America and the frontier was the future toward which he was always looking. He found endless fascination in it.

This book fills in the details of this fascination and is thus very useful for students of Western history. Few books are perfect, however, and this work falls short on at least two counts. Jackson devotes far too little space, in this reviewer's opinion, to Jefferson's crucial role in forging the concepts upon which the Northwest Ordinance was based. This, in many ways, was his most important service to the country because it solved the problem of empire by devising a way for colonies to become equal partners in the destiny of the new country as a whole. The fact that Jackson takes this somewhat for granted is symptomatic of another deficiency in the book. Detail overwhelms Jefferson's larger philosophical view of the role of the West and the nation in world history, a role of which he was acutely aware as the lofty language of the Declaration of Independence makes clear. Jefferson once said that he was "savage enough to prefer the woods," but he was also a cosmopolitan and world-historical thinker. Little of this Jefferson comes through in what is otherwise an excellent, highly interesting book.

WILLIAM H. GOETZMANN
University of Texas,
Austin

PAUL G. FALER. *Mechanics and Manufacturers in the Early Industrial Revolution: Lynn, Massachusetts, 1780-1860*. (SUNY Series in American Social History.) Albany: State University of New York Press. 1981. Pp. xvii, 267. Cloth \$34.00, paper \$9.95.

In two influential essays published in 1974 and 1975, Paul G. Faler introduced the categories of "traditionalist," "rebel," and "loyalist" to represent the broad cultural orientations of the workers of

nineteenth-century Lynn. "Traditionalists" clung to an older "libertine morality"; the other two groups embraced a new "industrial morality," only made different uses of it. What relationship these essays, or his 1971 doctoral dissertation on Lynn's shoe workers, have to the present book is unclear (since he never bothers to explain it). But if the book is not simply the dissertation warmed over, it shows some interesting new turns in interpretation.

The three categories have now disappeared, giving way to a more complex accounting of the growth of class consciousness among the shoe workers. For the half-century after the Revolution, the emphasis is on how thoroughly they shared with master craftsmen an identity as mechanics, both subscribing to "the mechanic ideology," which extolled the value of labor and the producer and vilified social parasites (no libertinism here). Journeymen expected, and by and large got, fair treatment from bosses whose circumstances and affiliations were remarkably similar to their own. Faler is most concerned with how, after 1830, this concord gradually broke down. While the shoe industry was not mechanized until the eve of the war, its structure did change radically, elevating shoe merchant over master, increasing the merchant's economic leverage and wealth, separating him physically from his employees. Simultaneously, the lives of shoe workers became more precarious and less rewarding. The big manufacturers (Quakers, mostly) mounted a vigorous campaign for moral reforms, such as temperance and a more rigorous poor relief, with an obvious class bias. They promoted new associations—a high school, a lyceum, clubs, even a cemetery—based on, or at least oriented toward, wealth, where laborers were made to feel unwelcome. This segregation of social intercourse by class prompted workers to form groups of their own, such as volunteer fire companies, cooperatives, and unions. More significant, it bred a festering distrust of the bosses. The mechanic ideology was turned by journeymen against their former brethren, now seen as deceitful exploiters. A constant sniping between the two sides culminated in the great and bitter strike of 1860, signaling the death of social harmony in Lynn.

Based mainly on exhaustive research in local newspapers, this is a welcome addition to the new labor history, especially in its careful delineation of the interplay between economic and social change. Yet carefulness can be inhibiting, too. With the interpretive scheme of the essays gone (no great loss, to my mind), nothing has been brought forth to replace it. The book lacks focus, merely meanders along in an earnest, somewhat plodding way. Perhaps the problem is a reluctance to break from earlier formulations. The "industrial morality," for instance, does make a cameo appearance here, an ill-named, badly conceived idea that should have

been retired. It is part of a lingering presumption that economic structure determines consciousness. If Faler had concentrated more on the ways shoe workers accepted the entrepreneurial, individualistic ethos of a society that was failing to make good on its promises to them, he would have come closer to the true tragedy of the antebellum working class.

CARL SIRACUSA
Connecticut College

RICHARD HOLCOMBE KILBOURNE, JR. *Louisiana Commercial Law: The Antebellum Period*. Baton Rouge: Paul M. Hebert Law Center Publications Institute, Louisiana State University. 1980. Pp. xix, 233.

Louisiana has been one of those states whose past legal historians have not well understood. Seemingly steeped in French civil law, its legal traditions appear to be separate from those of most American states. In an effort to untangle some of the mysteries of its state's legal history, the Louisiana State University Law Center has established the Center of Civil Law Studies, which in turn has hired a legal historian and begun to sponsor scholarly inquiries into the civil law legacy. This book represents one of the Center's initial historical enterprises.

Richard Holcombe Kilbourne, Jr., never clearly marks the chronological boundaries of his study, but it appears that it deals with certain developments in the commercial law of Louisiana from roughly 1825 to 1845. He discusses the origin, evolution, and abandonment of the proposed Commercial Code of 1825. He also treats the Commercial Court of New Orleans, which he contends was one of the last courts in the nation that dealt almost exclusively with commercial law. Major parts of the book likewise summarize the law of factors and brokers, and commercial paper. A final chapter concerns the impact of the depression of 1839 on Louisiana and several representative cases that arose from this unhappy event.

Although Kilbourne "endeavors to elucidate the importance of law in furthering commercial enterprise" (p. xv), he does not always succeed in this attempt. He devotes much space to the abortive Commercial Code of 1825 but never really tells us how this code would have facilitated commercial development. He demonstrates that there was a national body of general commercial law even before *Swift v. Tyson* and that the Louisiana judiciary adopted much of it, but he does not explain the economic significance of these developments. He offers an elaborate analysis of the judicially defined duties and liabilities of factors but does not suggest what impact these cases had on economic development. He convinces us that the bill of exchange was an important negotiable instrument and that when

ruling on these instruments Louisiana courts usually followed American and English precedent rather than French, but he does not adequately relate Louisiana court decisions to economic progress. He asserts that the Commercial Court of New Orleans (1839–46) facilitated commercial activity but spends too many paragraphs on the biography of the judge, the rules of court, and jury lists and not enough on its decisions and their significance. Finally, he offers an elaborate discussion of the collapse of the United States Bank of Pennsylvania and several resulting cases in the Commercial Court without explaining what influence these decisions had on either Louisiana or national commercial law.

Too much of the text is taken up by long quotations, some of which are from technical documents that might best have been summarized. Too much of the study represents but a synthesis of secondary scholarship and not the fresh findings and interpretation that a monograph on a rather narrow subject should contain. Contrary to Kilbourne's assertion, Chief Justice Marshall's opinion in *Gibbons v. Ogden* did not "den[y] the existence of state power to regulate interstate commerce" (p. 59). If it had done so, it would not have been necessary for Justice Johnson to have written a concurring opinion in which he disagreed with Marshall's refusal to opt for exclusiveness. This becomes a rather major error as the author submits that *Gibbons v. Ogden* constituted a significant influence on the evolution of Louisiana commercial law.

Kilbourne tells us much about the commercial law and practices of antebellum Louisiana and proves that they were more Anglo-American than French. He would have told us more had he created a more relevant historical context for his subject and taken more care to explain the implications of his findings.

ROBERT M. IRELAND
University of Kentucky

JOHN N. DICKINSON. *To Build a Canal: Sault Ste. Marie, 1853–1854 and After*. Columbus: Ohio State University Press, for Miami University. 1981. Pp. xviii, 204. \$21.50.

The Sault Canal, only a mile long and necessary because of the relatively slight twenty-one-foot difference between the levels of Lake Superior and Lake Huron, is one of the most heavily used man-made shipping channels in the world. Constructed by the St. Mary's Falls Ship Canal Company, an Erastus Corning enterprise, in return for the 750,000-acre federal land grant to the state of Michigan in 1852, the waterway was completed in May 1855. The first vessel transited the two-lock canal in June, the first iron ore cargo, eventually to surpass 100 million tons annually, passed through in

August 1855. John N. Dickinson has made a careful and useful study of the canal's construction, with particular reference to its legislative and complex financial history, including the disposition of carefully selected mineral and pine lands received by the construction firm.

Not enough effort was made to place Michigan's canal experience within the context of other internal improvement programs, and often, because of the lack of general and summary statements, it is difficult to follow the author's narrative, but the main outlines of an intriguing and important development are present. The major figures in the story are Charles T. Harvey, a Fairbanks Scales Company salesman turned project manager whose imagination sparked Eastern involvement in the enterprise but whose inexperience and testiness later nearly wrecked it; a Sault Ste. Marie newspaper editor, the "treacherous" (p. 86) J. Venen Brown; and a trio of railroad men—financier Erastus Corning, attorney James F. Joy, and engineer John W. Brooks—who oversaw the project and carried it through to completion. To Brooks, in fact, after Harvey had bungled the job, is given "the major credit for the fulfillment of the contract" (p. 128). Dickinson has traced their activities during the crucial construction years with sometimes mind-numbing detail, but there emerges a sense of the drama, the interplay of personalities, and the unsung heroism of hundreds of workers battling frigid temperatures, ice-choked channels, and the seemingly inevitable cholera epidemic.

Curiously, there is little discussion of the canal in operation. Instead, a brief epilogue (pp. 168–71) alludes to the delayed economic impact of the canal because of the need for improved human and technological resources in both directions from the canal before its worth could be realized; additionally, some attention is paid to the "costly" social impact of the canal lands. When the state of Michigan, under the terms of the Morrill College Land Grant Act of 1862, received 240,000 acres of the public domain, the amount of good quality timber or mineral land available had been severely depleted by the astute selections of the dozens of "land-lookers" hired by the construction company in the 1850s. Nevertheless, following emphases developed by Irene D. Neu in her pioneering essays on this topic, Dickinson's book is a valuable addition to the bookshelf of works dealing with individual internal improvements.

RALPH D. GRAY
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ADRIENNE SIEGEL. *The Image of the American City in Popular Literature, 1820–1870*. (National University

Publications Interdisciplinary Urban Series.) Port Washington, N.Y.: Kennikat. 1981. Pp. 211. \$15.00.

There is a wide variety of documents that detail the urban explosion in the United States between 1820 and 1870. The familiar evidence ranges from population counts to individual firsthand observations and impressions of rapidly growing cities. Adrienne Siegel finds yet another, less recognized reflection of intensified urbanization in the flood of city fiction surging through the urban centers of the United States during these decades. She stresses that the mere trickle of 18 books produced in the fifty years between 1770 and 1820 in the next fifty years swelled to tidal waves inundating the American market with 20 new books in the 1830s, and with 173, 167, and 97 in each of the subsequent decades. A substantial portion of the literary output, selected at random, as well as exposés and plays of the big city form the core of the material that makes up her survey of popular urban literature.

The study establishes the familiarity of many writers and the fascination of countless readers with the varied urban scene. It then presents aspects of the success and failure of city people, their hopes and frustrations, and their dreams and realities. The material and spiritual framework used for topical arrangements of facts and fancies are wealth and poverty as well as sin and virtue. These convey quite effectively not only the general tenor of city novels about the pleasures and strains of urban life but also the specific conditions of ethnic groups, laborers, and women. Special attention is given to the characteristics of certain individual cities, particularly to New York, which furnishes the setting for almost half of the tales. For each topic, such as modernity, economics, labor, social welfare, housing, politics, race, sex, and literature, references to the pertinent secondary literature provide analytical contexts to buttress and broaden the significance and understanding of fiction as historical document.

An approach linking the materials to the relevant scholarship of the respective areas hardly leaves space for an independent treatment of the information. It supports a tradition among students of the past who have rarely found anything they were not looking for and who limit imaginative uses of fiction and art when they consider reality of greater importance than accepted reflections of it. The study concludes that popular books failed "to come to terms with the city" (p. 177), but the study itself fails to pull together the many bits and pieces of insights from pulp literature about the urban scene into the image of the American city, as the title of the book promises.

Adrienne Siegel's forte is her intimacy with incidents and fantasies in the popular literature of the day that relate people's struggles with or accommo-

dation to distinctly recognizable features of modern city life. She gathers these details assiduously, links them to established concerns carefully, and probes the psychological functions the stories served for the minds of their readers. As the product of her methodical tracing of a genre of evidence on urbanization, all too often used merely to provide color or to enliven the past, we now have a good survey of an important segment of American city fiction.

GUNTHER BARTH
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STANLEY B. KIMBALL. *Heber C. Kimball: Mormon Patriarch and Pioneer*. Urbana: University of Illinois Press. 1981. Pp. xv, 343. \$17.95.

One gains much of Mormon history, from the founding of the Church of Jesus Christ of Latter-day Saints in 1830 in New York state to 1868 in Utah Territory, in this, one of the better biographies of Mormons to appear in recent decades. Heber C. Kimball, one of the early church's leading figures, is important for his closeness to important events, as one of the Twelve Apostles and "father" of the British mission, and for his closeness to Brigham Young (friends before they accepted Mormonism, and counselor to him from 1847 to 1868) and hence in the power center after 1835.

The major contributions of this book are found in its treatment of Kimball's participation in the Mormon exodus from Nauvoo, and in the overland treks of 1847 and 1848, and as a much-married polygamist and father.

Stanley B. Kimball is an expert on the Mormon Trail and overland experience, and his chapters on the 1846 trek across Iowa and the 1847 pioneer company's journey to Salt Lake Valley and return have the makings of the best account in print of those experiences, kept from being the best by some stylistic problems and by the obvious necessity of keeping close to his subject. Still, the author's store of knowledge enriches his narrative.

Heber C. Kimball, by the author's count, had forty-three wives; seventeen bore him sixty-five children, forty-three of the latter lived to maturity. The inauguration of plural marriage among the Mormons is well documented, and chapters on the "Reluctant Polygamist" and "Kimball at Home" give an unvarnished look into the practice. There is no whitewashing of personal or family affairs.

The work is based on solid research in previously unavailable primary sources of both individuals and the church and makes use of distinguished monographs produced during the past generation. Notwithstanding the general excellence of the work, some characteristics bother this reviewer. While we

are spared the fault of loading footnotes and bibliographies with superfluous and repetitive citations, to limit the "Bibliographic Note" to fewer words than appear in this review, and give us mainly generalizations relative to primary and secondary sources used, is excessive. Chapter footnotes relate mainly to quotations. The author waves erudition before us, but misses some of the subtleties of the background history, while controversial matters in that history are resolved with the flourish of the pen or run around quickly without documentation. The author's aim to appeal to both Mormon and non-Mormon readers has led to the practice of continual asides and commentary throughout the essay. A critical reading by an uninhibited good copy editor could have improved the literary style and flow of the narrative.

S. GEORGE ELLSWORTH
Utah State University

TERRY G. JORDAN. *Trails to Texas: Southern Roots of Western Cattle Ranching*. Lincoln: University of Nebraska Press. 1981. Pp. xv, 220. \$15.95.

Terry G. Jordan is a cultural geographer, a Texan, and a descendant of people who migrated from the Carolinas to East Texas with their livestock in the early nineteenth century. Two decades of research in the history of the Western range-cattle industry have convinced him that the experience of his ancestors was not atypical. *Trails to Texas* attempts to document the diffusion of Carolina cattle culture through the coastal plains and pine barrens into the West (or the part of it that is East Texas) and to measure the Southern influence on what is often thought to be a Western institution. Jordan contends that Western open-range ranching is a hybrid form, produced by the mingling of Carolina (and thus partially African) and Hispanic cattle cultures. The degree of hybridization varied from southeast Texas, where it may have begun as early as 1820, to northeast Texas, where there was little or no Hispanic influence on ranching before 1848.

In this book, Jordan has gone against the fashion of the last fifteen years in writing about the West, even if his is a bland corrective to the prevalent notion that each minority group must be represented in substantial numbers in every retelling of some significant or interesting Western enterprise. Jordan found that only 4 percent of cowboys in West Texas in 1880 were black, a number considerably below the 15 percent figure offered as guesswork by Philip Durham and Everett L. Jones in 1966 but nevertheless quoted ever since as substantiated fact in monographs and textbooks alike. Jordan suggests also that Indians contributed nothing Indian to cattle raising, since most of those in Indian Territory were

originally from the South anyway and thus merely served as additional carriers of Anglo-American culture. And, of course, the Hispanic influence was not nearly as profound as has been formerly believed, Jordan argues.

Jordan does not discuss cattlemen's associations or their presumed antecedent, the Mesta, be it Spanish or Mexican; and he does not consider the westward spread of cotton culture in connection with cattle, though both agricultural forms covered the same geographical area, albeit at perhaps slightly different times. The interested reader will be sorry not to have Jordan's views.

Jordan's work is as persuasive as it is passionless. Still, it is a view of the South from the West, to the extent that his research centered almost exclusively on Texas. Among his primary materials are a few travelers' accounts of the South of the sort often reprinted and easily found, but whereas Texas manuscript collections are well represented, there is not a single item from any archive in the lower South. I do not suggest that such materials would contradict Jordan's findings—indeed, quite the contrary. But when one sets out to examine origins, one should, it seems to me, give some indication of having seen them.

WILLIAM W. SAVAGE, JR
University of Oklahoma

MILTON RUGOFF. *The Beechers: An American Family in the Nineteenth Century*. New York: Harper and Row. 1981. Pp. xvii, 653. \$19.95.

"This country," goes Leonard Bacon's familiar quip, "is inhabited by saints, sinners, and Beechers." Milton Rugoff has dauntlessly corralled that Beecher third of the nation—the Reverend Lyman and his eleven-odd sons and daughters—into a long but eminently readable and insightful "family saga." It is not that we have lacked first-rate studies of members of the clan. Kathryn Sklar's biography of Catharine, the work of William McLoughlin and Clifford Case, Jr., on Henry Ward, as well as Marie Caskey's study of the varieties of Beecher religious experience, among others, have illuminated important themes in the social and intellectual history of nineteenth-century America. No one until Rugoff, however, has combined the inspiration and fortitude necessary to create a comprehensive group portrait.

The Beechers, aimed at a lay audience, emphasizes narrative values. Rugoff presents his characters in overlapping, chronological biographies, intertwining them through key personal and historical events. Certain familial traits provide additional threads of unity. For instance, Rugoff captures that peculiar Beecher brand of moralism—a flinty cock-

sureness that wavered somewhere between the sacred and the profane, but rarely approached the profound. He also uses the family to illustrate the shift in American ideas of goodness and morality from Lyman's neo-Puritanism to Henry Ward's more congenial, if somewhat self-deluded, brand of liberal Christianity. Along the way, Rugoff ably portrays the many theological disputes and historical movements in which various Beechers found themselves embroiled. He has combined intimate knowledge of the manuscripts with wide reading in the latest historical literature to produce a model of popular history.

Though historians of the period will find no startlingly new "thesis" here, the book does have its rewards for the scholar. Rugoff has enriched otherwise unsurprising characterizations of the major Beechers with subtle turns and fascinating details and gives us vivid new portraits of the minor ones. Only in one respect does *The Beechers* disappoint. Rugoff underplays what surely must have been one of the most powerful forces in the Beecher household, the subtle and sometimes not so subtle tension between love and competition among the children.

Nonetheless, for the first time we have a volume that lays out the full cast of characters necessary to an exploration of this and other important themes related to Lyman and his brood. That Rugoff accomplishes such a task with grace and deep sympathy, engaging us for six hundred pages, is perhaps recommendation enough for this delightful book.

ROBERT H. ABZUG
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Austin

ANNE C. LOVELAND. *Southern Evangelicals and the Social Order, 1800–1860*. Baton Rouge: Louisiana State University Press. 1980. Pp. x, 293. Cloth \$30.00, paper \$12.95.

Anne C. Loveland's thoughtful book recounts the conversion, calling, and training of Presbyterian, Methodist, and Baptist ministers in the antebellum South, and analyzes the interplay of their social concerns with their religious beliefs. It fills admirably a long-standing gap. She has read carefully an immense range of books, pamphlets, and periodical literature, and dozens of collections of private papers. She manages, moreover, both to let these records speak for themselves and to tell us in non-nonsense language what she believes they mean. The book is a harbinger of the new understandings now open to those who will follow the trails that Loveland, Donald Mathews, and Brooks Holifield have recently blazed.

Loveland's descriptions of the psychology of con-

version and of the spiritual nature of the "call" to the ministry establish the close similarity of the religious experiences of pastors in these three evangelical sects. She demonstrates that the majority understood revivals to be God given as well as humanly planned events, and that opposition to them stemmed from fear of relying too much upon human efforts to accomplish God's work. This sets the stage for her discussion of how their antipathy to "worldliness" rationalized both the prophetic social judgments some of the ministers made and the "spiritual" withdrawal of others from all efforts at reform.

Wisely postponing discussion of the issue of slavery, Loveland turns first to the temperance crusade, finding it a model of the desires and dilemmas of those who longed for a Christian society. Opponents of total abstinence found Southerners far more responsive than Northerners to the argument that the literal statements of Scripture and the practices of biblical times must determine nineteenth-century Christian behavior. Resistance to the temperance crusade, as to prison, sabbath, and other movements of "benevolence and reform," also drew upon what the author concluded was a general evangelical suspicion of human effort, and upon James H. Thornwell's doctrine of the "spiritual church," which became popular after 1840. I believe that suspicion and Thornwell's doctrine were pervasive, however, only among conservative Calvinists; Methodists found little place for them.

The wisdom of these earlier chapters inspires confidence in the later ones on slavery. They present a richly detailed story of the reaction of evangelical clergymen to the growing power of the institution, then to the crisis posed by radical abolitionism, and, finally, to the emergence of a proslavery and quite secular nationalism in the South. Preachers began very early to emphasize the "biblical" injunction to respect lawful authority and to stress both God's sovereignty and human helplessness to deal with such a great evil. Then, in the mid-1830s, many of them welcomed the "discovery" that the Bible sanctioned the existence of the slave system, despite its injustice.

In this context, the crusade for the religious instruction of Negroes, in which Methodists were clearly the leaders, seemed to its advocates an admirable application of New Testament teachings on the moral responsibility of masters to slaves. Loveland's conclusion, however, is unequivocal. The rejection of Christian responsibility to perfect the world muted all direct challenge to the injustices inherent in the system and tended to perpetuate them. Moreover, in the crisis of the late 1850s the rhetoric of depending on God, rather than human beings, for judgments that would be "true and right-making altogether" paved the way for Christians to

embrace violence. In both the North and the South, they joined the failed politicians in blaming the carnage on a warrior God, who was "trampling out the vintage where the grapes of wrath are stored."

I think Loveland's book would have been even more powerful if she had pursued further the theological and social differences between the three "evangelical" communities. That rubric, undefined, is still used too freely in the analysis of both nineteenth-century and current history. It obscures the immense diversities of theological tradition, biblical understanding, structure and styles of leadership and worship, and attitudes toward this world and the next that characterize the mosaic of movements that rightly bear the title "evangelical." Presbyterians, far less numerous, seem peripheral in this study. Is this not largely because, as the author states clearly only once, their Calvinism, and that of Southern Baptists, remained largely unaffected by the Americanized "new divinity" that Congregationalists and Presbyterians embraced in the North?

The reaction against "human means" and the doctrine of the "spiritual church," which Southern Calvinists adopted, contradicted the entire history of Puritan political and social involvement in England and America. It beclouded the memory of the revivals set in motion in the South by George Whitefield and Samuel Davies. And it required a tragic distortion of their own history and theology. Likewise, the Methodist renunciation of radical hopes for an early end to slavery in the South, and for the complete exclusion of it among its own members, represented a compromise with views of both Christian perfection and social justice that John Wesley's preachers had planted deep in American Methodist consciousness during and after the Revolutionary War. That, as Loveland points out, both Wesleyans and Calvinists came by the 1840s and 1850s to regard abolitionists as social, economic, and religious radicals, required them to bury the memory of antislavery convictions they had held only a decade or so earlier.

TIMOTHY L. SMITH
Johns Hopkins University

LEONARD P. CURRY. *The Free Black in Urban America, 1800-1850: The Shadow of a Dream*. Chicago: University of Chicago Press. 1981. Pp. xix, 346. \$25.00.

This is the "first fruit" of Leonard P. Curry's long-term comparative study of the nation's fifteen largest cities in 1850—a prodigious attempt to analyze "the American urban experience in the first half of the nineteenth century." Although free blacks were a minuscule group in Northern cities, they constituted a substantial segment of the residents in Washington, Charleston, and New Orleans. In the course

of pursuing this broad-gauged research project, Curry decided there was sufficient material for a half-century "synthesis" of the experiences of free blacks in these cities, and indeed in the present volume he offers a vast array of data (some trivial or undeveloped) on subjects like black urban welfare, education, religion, and associational activities.

Curry has written first-rate chapters on comparative black occupational and residential patterns based on exhaustive examinations of manuscript census and city directory sources in such a clear and cogent manner that nonquantitative historians will find them especially useful. He vividly shows that nearly all "slave area" cities offered free black males greater opportunity to enter artisan trades, a situation he explains by the fact that traditionally, since so many slaves in the South were skilled workers, the presence of free black artisans seemed less threatening to whites than it did in the North. Moreover, these Southern free black artisans provided apprenticeships to the future generation of black males, thus enabling them to enter trades. Curry's chapter on antebellum black residential patterns is similarly outstanding, and nowhere has this reviewer seen a better discussion of exactly what is known and unknown about this complex subject. Curry points out that, although "recognized black districts" existed in all these cities (generally with higher racial concentration in Northern centers than in Southern ones), yet it was the proximity to white neighbors in the antebellum city that was so different from the black experience in the twentieth-century city.

Unfortunately, for all its merits, the book ignores topics like black families and households, black leadership and ideology, and the relationship of blacks to the class of wealthy whites. Many topics that are included such as black participation in politics are treated statistically and not very deeply. In Boston where blacks had considerable political opportunity, Curry identifies a black voting bloc that appeared as early as 1800. He notes that in the 1845 election 357 black Bostonians cast ballots but presents nothing about the degree to which this political activism operated either to gain tangible favors or to ameliorate racial discrimination. Likewise the chapter on associational activities is thin. For example, the author has credited the African Society for Mutual Relief with "inordinate influence" in black Manhattan but does not show how this influence was manifested. He mentions other organizations such as the New York Association for the Political Elevation and Improvement of People of Color, but there is no discussion about how they functioned.

The volume, in spite of its virtues, has an encyclopedic and somewhat superficial quality. First and foremost, an in-depth study of the black experience

in one city is a major task, and it is simply impossible for one person to do adequate research on fifteen. In addition, the limited nature of both the quantitative and more traditional kinds of black sources for the antebellum period poses serious obstacles for historians, a fact amply demonstrated by at least three studies, published and unpublished, of blacks in Boston before the Civil War. In any event, several discrete, competent studies of individual cities are imperative before historians can turn to the task of making an adequate synthesis.

Nevertheless, Curry's book contains a great deal of useful data for future scholarship.

ELLIOTT RUDWICK
Kent State University

MARK V. TUSHNET. *The American Law of Slavery, 1810–1860: Considerations of Humanity and Interest*. Princeton: Princeton University Press. 1981. Pp. viii, 262. Cloth \$20.00, paper \$9.50.

This study breaks important new theoretical ground in the increasingly rich analysis of the American law of slavery. Mark V. Tushnet probes the opinions of antebellum Southern state appellate court judges in order to explain the unresolved legal tension between the slave as human being and as property. Tushnet suggests that the master class struggled to reap the rewards of a bourgeois economy without falling prey to its radical individualism. Southern appellate judges, he concludes, most frequently wrote for internal consumption rather than to rebut the arguments of Northern abolitionists, and they did so with the goal of developing a "categorical approach" to slave law as the best means of "reconciling the demands of humanity and interest" (p. 10).

Tushnet contributes significantly to the existing literature by using Marxist theoretical constructs to argue that Southern appellate judges never resolved the contradiction between sentiment and interest. When confronted with the mixture of slave and market relations in criminal, contract, tort, and manumission cases, these judges authored opinions that fell short of a categorical approach to slave law. He offers a variety of explanations for this failure: the common law system forced judges to labor under the weight of precedent and to fear the implications of slave case decisions for free whites; and the history of slavery was sufficiently brief and the judges' racist predilections sufficiently strong to preclude the development of a coherent body of slave law. But the most important reason was the level of judicial talent—the judges were "merely ordinary thinkers" (p. 7). These jurists, Tushnet concludes, usually lacked the intellectual capacity to

manipulate legal categories in ways that would allow a decisive break with bourgeois practices.

Tushnet captures theoretical coherence through a severe homogenization of geographical, chronological, and judicial variables. Of the 191 cases he cites, fully 85 percent are drawn from five jurisdictions—Alabama, Georgia, Louisiana, Mississippi, and North Carolina. While insisting that his purpose was to write a “sort of ‘Restatement of the Law of Slavery,’ ” and that the cases he draws on were to be “illustrative and not typical,” he does, by the nature of his argument, suggest that, local variants aside, these were representative cases. But representative of what, and in what time period and of what judges? There are, for example, no border state cases (save one from Delaware). Tushnet says that he read all of the cases contained in Helen Caterall’s *Judicial Cases Concerning American Slavery and the Negro* but that he decided to disregard the decisions of the courts of Tennessee and South Carolina because of “the volume of the material” (p. 233). The preponderance of the cases are drawn from the period 1851 to 1861; fully 58 percent come from these years and fully 78 percent from the period 1841 to 1861.

Homogenization produces its most serious casualty in the treatment of the judiciary and the judicial process. Analytical prowess is not an exclusive component of judicial ability; judges often row to their objectives with muffled oars. This narrow view of judicial ability begs the question of the nature of judicial decision making and trivializes judicial motive. Tushnet, for example, may have mistaken lapsed logic (either through commission or omission) for *purposeful* ambiguity. The author measures the ability of these judges by what they should have done to categorize the law of slavery, a goal that he does not demonstrate they shared.

Because of its narrow conception of judicial ability, its concentration on judicial opinions, and its homogenization of geography and chronology, *The American Law of Slavery* warrants attention more as an evocation of historical theory than as an arresting exposition of the law of slavery. It is, nonetheless, a vital contribution to the literature.

KERMIT L. HALL
University of Florida

FRED BATEMAN and THOMAS WEISS. *A Deplorable Scarcity: The Failure of Industrialization in the Slave Economy*. Chapel Hill: University of North Carolina Press. 1981. Pp. xiii, 237. \$19.00.

“In the economic histories of the South,” write Fred Bateman and Thomas Weiss, “manufacturing has occupied the lowest position of all. In the scant literature on the subject, the most frequently cited characteristic of antebellum southern manufac-

turing is its alleged backwardness” (p. 4). Their book, *A Deplorable Scarcity*, does little to refute this picture of antebellum Southern manufacturing; however, in the process of exploring this backwardness, Bateman and Weiss provide us with a much firmer base on which to judge the performance of Southern manufacturing.

This book is the product of more than a decade of quantitative research. Whether or not one agrees with the interpretations put forward, any researcher must be impressed with the statistical evidence that Bateman and Weiss have assembled. They have developed a new data source through the construction of two samples drawn from the census manuscripts of manufactures for each of the census years 1850, 1860, and 1870. The samples and discussions of the methods in which the data was used to empirically test specific issues, are discussed in the appendixes.

Bateman and Weiss’s basic quantitative finding is that antebellum Southern manufacturing *was* profitable. “The average for all southern manufacturing,” they conclude, “was 28 percent in 1860, and 25 percent in 1850” (p. 106). This conclusion seems quite convincing. It raises another question, however, which Bateman and Weiss try to answer: Why did Southern planters not invest in these profitable ventures?

Recent studies of the antebellum South have tended to stress the region’s large comparative advantage in cotton agriculture as the reason for the South’s negligible attention to manufacturing. The problem with comparative advantage, according to the authors, is that manufacturing was *more* profitable than slave agriculture. What to make of this? Having considered several other explanations, Bateman and Weiss reject comparative advantage in favor of another, equally familiar, theme: “The answer appears to lie with the planter class, specifically in its members’ attitude toward risk” (p. 160). Southern industry, in other words, did not grow because of a lack of entrepreneurial ability.

The observation that firms that were actually doing business were “profitable” does not mean that most (or even many) Southerners would have been better off investing their capital in slaves and factories rather than in slaves and cotton land. While high returns imply that opportunities for investment in industry existed, it may have been the case that returns in Southern manufacturing were high precisely because the industrial sector was so small. Bateman and Weiss insist that the Southern market could have sustained expansion. Yet their evidence suggests that Southern firms were small, and typically oriented toward a local market. Even a modest expansion of manufacturing might have caused the observed high returns to fall dramatically.

This points up a major difficulty with Bateman and Weiss’s argument. “Our work,” they admit,

"may be largely an industrial history without industrialists, but despite their anonymity these actual or potential manufacturers are never far below the surface" (p. 163). The question of why so many of the manufacturers never surfaced is at best conjectural. The evidence on agricultural entrepreneurs, by contrast, is readily available. It suggests that the "cautious, slow-moving southerner of literary tradition" (p. 163) whom the authors blame for the South's failure to develop an industrial sector did not seem reluctant to pursue agricultural ventures, even when those ventures involved both risk and large amounts of capital. Why the bias against investment in industry?

The answer, it seems to me, is that the comparative advantage thesis is *not* inconsistent with the evidence on manufacturing profitability. If the returns to cotton had been stagnant or falling or if the returns to slaves in agricultural ventures had been yielding returns well below the 6 to 8 percent generally regarded as "normal" at that time, then Bateman and Weiss might have a case for entrepreneurial neglect. But we know that this was not the case. Slaves, land, and a growing market for their product (the fruits of comparative advantage) went hand in hand with a lifestyle and social structure (the plantation system and planter class) that seemed quite attractive to Southern investors.

In the final analysis, Bateman and Weiss accept this point, conceding that: "Even if investors had seized quickly upon industrial opportunities . . . the South might still have seemed underindustrialized to outside observers. The region's factor endowment would still have led to an industrial sector that was small relative to that in the East" (p. 163). This is a fitting conclusion for this study. *A Deplorable Scarcity* is a well-written, neatly argued book that offers some good insights into the issues of industry in the antebellum South. Historians of both the antebellum and postwar South should find it valuable.

ROGER L. RANSOM
University of California,
Riverside

BRUCE COLLINS. *The Origins of America's Civil War*. New York: Holmes and Meier. 1981. Pp. 169. Cloth \$22.00, paper \$13.50.

Confessedly "pedagogical" (preface), Bruce Collins's *The Origins of America's Civil War* represents an effort to explain the disruption of the Union in a brief synthesis based on the important monographs of the past two decades. Older works and several journal articles are cited, but specialists will not need the footnotes to comprehend how much Collins owes to the insights of recent books by such scholars as David M. Potter, Michael F. Holt, Eric Foner,

Richard H. Sewell, J. Mills Thornton III, William J. Cooper, Jr., and Ronald P. Formisano.

Collins renders an interpretive analysis. He sides with historians who have argued the essential political and cultural commonality of Southerners and Northerners in the antebellum era, thus rejecting the suggestion of a precapitalist-bourgeois dichotomy common to the works of Eugene D. Genovese, Raimondo Luraghi, and Emory M. Thomas. Collins believes that both regions were "middle-class democracies" (p. 7). He attributes Civil War causation primarily to "federal policy concerning slavery extension and the *long-term* implications of that policy" (p. 7). Slavery expansion commands center stage, in contradistinction to several recent studies such as Michael P. Johnson's *Toward a Patriarchal Republic* (1977). In fact, despite a stated intention to steer clear of "historiographical debates" (preface), Collins singles out Johnson's class interpretation of secession for explicit criticism. Many readers will find Collins's geopolitical theme a refreshing change of historiographic direction; others will take strong issue.

Brevity constitutes the book's most serious limitation. Collins allocates only a few sentences to John Brown's raid. Sometimes he makes significant omissions. Thus, he simplifies the Southern expansionist impulse toward the American Southwest, failing to recognize that interest in applying slave labor to mines complemented aspirations to extend plantation agriculture. Nowhere does he note William L. Barney's evidence that intimidation undermined Unionist voting in secession convention elections. Further, he portrays William Seward as pressuring Lincoln toward war with Britain over Canada as a means of restoring the Union; actually Seward's notorious April 1 memorandum, while mentioning Great Britain, threatened war against Spain and France in response to European interventionism in Mexico and Spanish reannexation of the Dominican Republic. Collins ignores Midwestern determination to maintain navigation of the Mississippi River as a contribution to Northern insistence on maintaining the Union. Such deficiencies detract from an otherwise sound treatment.

The Origins, however, fulfills its goal of providing an introduction to the causes of the war. Concise, well-organized, subtle and sensitive to recent research even when in dissent, the narrative provides effective summaries of North-South differences, the centrality of land mobility to antebellum Americans, abolitionism and political antislavery, the proslavery ideology, sectional crises, and the reasons for the triumph of the Republican party. Unfortunately, an excessive price will limit its obvious potential for classroom use.

One final caveat. Collins further reflects the state of the profession by devoting but a few token pages to why the North chose to fight in 1861. This

reminds us that when explaining the Civil War, historians unfortunately continue to regard the raising and commitment of the Union army as of infinitesimal importance when measured against the secession process.

ROBERT E. MAY
Purdue University

OTTO H. OLSEN, editor. *Reconstruction and Redemption in the South*. Baton Rouge: Louisiana State University Press. 1980. Pp. v, 250. \$17.50.

Reconstruction and Redemption in the South is a collection of well-written and most interesting essays describing the course of Reconstruction in six Southern states. Written by six different historians who are the pre-eminent authorities for their particular states, the articles depict the variety and diversity of the Reconstruction experience in the South.

Like the events they describe, the contributions vary, and they differ in both style and quality. They range from the narrative, descriptive treatment of Mississippi by William C. Harris, through the more interpretative pieces on Florida by Jerrell H. Shofner, Alabama by Sarah Woolfolk Wiggins, and North Carolina by Otto H. Olsen, to the most opinionated essay of all—Joe Gray Taylor's on Louisiana Reconstruction, which he regards as "an abject failure" (p. 205). Of these, the most illuminating and subtle is Otto Olsen's on North Carolina, while perhaps the most interesting of them all is one not yet mentioned, Jack P. Maddex's piece on Virginia.

Maddex's essay is noteworthy because, unlike the others, it tries to clarify the process of Reconstruction rather than explain its eventual collapse and failure. His essay, furthermore, is conceptually imaginative, since it employs a device that Maddex calls "centrism" to explain how the Republican and Democratic parties were set aside by the creation of a single organization, the Conservatives, consisting of moderates, or "centrists," from both of them. This thrust toward the political center was actually at work in all the Southern states early in Reconstruction, but only in Virginia did it result in realignment and a one-party hegemony.

Nevertheless, with the exception of Maddex's essay and also Harris's, there is a uniformity of interpretation that does offset the obvious differences of style and quality. For the majority of the authors are preoccupied with accounting for the failure of Reconstruction, and they conclude that, essentially, the blame should be placed on the Southern Republicans themselves. Their political moderation and timidity as well as their failure to heed and protect their largest and most loyal bloc of constituents, the blacks, prevented them from building a viable party. Because of this, the leadership

was never able to develop a positive agenda and so, as Otto Olsen remarks, "they seemed to have nowhere to go and were constantly on the defensive" (p. 196). This highly critical view of Southern Republicanism has permeated Reconstruction historiography in recent years and it is amply confirmed by this collection. No doubt, its emergence is part of the current denigration of liberalism, but its effect is to make the tone of Reconstruction scholarship as denunciatory and as unsympathetic as it used to be under the New South—Dunning dispensation—but without even the compensation that the liberal and progressive forces could have made a difference if allowed greater opportunity.

A one-volume collection of brief, synoptic state studies like this is a most useful compendium for students of Reconstruction. But perhaps its chief contribution, and this Otto Olsen himself anticipates in his editorial introduction, may be that it will provoke rather different emphases in future scholarship from the one the book itself adopts. For example, future historians may be attracted by Jack Maddex's suggestion that the process and operations of Reconstruction be studied rather than its failure and the causes for it. A second outcome could well be that historians will employ the comparative state-by-state study of Reconstruction, not to emphasize interstate diversity and unrelatedness, but to isolate common features that are significant and determinative in the entire episode. If something like this should happen, *Reconstruction and Redemption in the South* may be the culmination of an existing trend and the point of departure for a new one.

MICHAEL PERMAN
University of Illinois,
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RICHARD C. CORTNER. *The Supreme Court and The Second Bill of Rights: The Fourteenth Amendment and the Nationalization of Civil Liberties*. Madison: University of Wisconsin Press. 1981. Pp. xi, 360. \$29.50.

This book succeeds in offering a fresh account of one of the most fundamental and familiar themes in modern constitutional history—the nationalization of the Bill of Rights. The essential elements of the story have long been known. They have, however, been dealt with in a disjunctive way, in judicial biographies and special studies of particular aspects of the Bill of Rights. As a result, the historical unity of the subject has been obscured. Oddly, it is a political scientist, Richard C. Cortner, who now brings together the separate strands of this constitutional development into a coherent and illuminating historical account.

The nationalization of civil liberties refers to the

process by which the Supreme Court has transformed the prohibitions and restrictions of the Bill of Rights, originally applicable only to the federal government, into limitations on the states. The court has done this by interpreting the rights enumerated in the first eight amendments as attributes of the liberty that is protected against state infringement by the due process clause of the Fourteenth Amendment. Cortner's narrative relates the steps by which this result has been accomplished: from the Supreme Court's nineteenth-century denial, in the *Slaughterhouse* cases and *Hurtado v. California*, of any correspondence between the Constitution's separate prescriptions of rights; to the transitional *Gillow* case of 1925, in which the court casually announced its assumption that the free speech guarantees of the First Amendment were subsumed under Fourteenth Amendment liberty; to the *Near, Stromberg*, and *Powell* cases of the 1930s in which parts of the Bill of Rights first provided the decisional basis for striking down state laws.

By 1937 the relationship between the Bill of Rights and the Fourteenth Amendment had gotten complicated enough, as a result of decisions and dicta spanning half a century, to warrant a more systematic rationalization. Justice Cardozo attempted to do this in *Palko v. Connecticut*. He presented a theoretical framework to explain why some provisions of the Bill of Rights had been absorbed into the Fourteenth Amendment and others had not. It was simply that some rights were more important than others, or, as Cardozo expressed it, certain rights were "implicit in the concept of ordered liberty," which the Fourteenth Amendment was intended to protect against denial by the states. Cardozo's theory of selective incorporation not only explained previous actions of the Supreme Court in applying national guarantees against the states but also pointed the way to further absorption of the Bill of Rights into the Fourteenth Amendment in the future.

In the 1940s and 1950s nationalization of civil liberties proceeded, albeit modestly, under the "fair trial" standard that was derived from Cardozo's *Palko* opinion. This test asked whether a right at issue in a state criminal proceeding—the focal point of efforts to nationalize the Bill of Rights at this time—was essential to a fair trial. It acknowledged a *similarity* between some rights guaranteed in the Bill of Rights and those secured by the Fourteenth Amendment, thus allowing for diversity in the states' precise definition of fundamental rights. Impatient with this approach, the Warren Court in the 1960s insisted on an exact *identity* between Bill of Rights guarantees and the Fourteenth Amendment. It employed the doctrine of selective incorporation, also derived from Cardozo's seminal opinion, to apply almost all of the restrictions of the Bill of

Rights against the states. Although Cortner properly emphasizes the differences between the "fair trial" and "selective incorporation" approaches to the problem, he shows that after the political reorientation of the judiciary in 1937 even judicial conservatives such as Felix Frankfurter joined in the task of imposing national civil liberties standards on the states.

In a concluding chapter Cortner reflects on the facts he has so assiduously presented. What stands out is the apparently adventitious nature of the decisive steps the Supreme Court took in the 1920s toward conflating the Bill of Rights and the Fourteenth Amendment. Cortner offers no new insight into the reasons for this momentous shift in legal doctrine, nor does he assay the larger significance for American constitutionalism of the libertarian changes that are his concern. We are left to consider the apparent paradox of an increasingly centralized and sovereign federal government creating a centralized system of civil liberties guarantees. Historians will nonetheless be in Cortner's debt for the comprehensive and accurate description he has provided of the legal changes by which civil liberties have effectively been nationalized.

HERMAN BELZ
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College Park

ELIZABETH RAUH BETHEL. *Promiseland: A Century of Life in a Negro Community*. Philadelphia: Temple University Press. 1981. Pp. xvii, 329. \$16.95.

Promised Land community ("Promiseland" was the popular rendering) originated during Reconstruction when land reformers in South Carolina subdivided a large plantation in what is now Greenwood County into small farms and sold the farms to freedmen. Using local records and interviews, Elizabeth Rauh Bethel has reconstructed major aspects of the history of this community and produced the most extended study now available of a group of blacks who acquired land during Reconstruction. The details of Bethel's story have the charm and concreteness that make good local studies intensely human, intrinsically appealing, and especially valuable for social historians. At her best, when detailing life at Promised Land, Bethel is both interesting and illuminating. The intricacies of land transactions over the years, the vignettes of individuals and families illustrating routines of life in the community—these and other such details make the book well worth reading.

But Bethel is not satisfied to tell the story of Promised Land. She has overlaid her story with an intrusive and not always helpful analytical apparatus and lessened its impact with a number of interpreta-

tions of questionable merit. A comparative approach and concern for the implications of some of her statements for other black communities might have made her more wary. Bethel finds much that is positive in Promised Land—a viable community life, strong family bonds, “stable and inherently conservative” institutions, “economic security and independence,” a hardworking, resourceful, and proud people—and accounts for these things by the large degree of land ownership. Yet these are the same things other scholars are finding in black communities where home and land ownership was far lower than at Promised Land. Likewise, Bethel blames the pattern that she describes among sharecroppers in the community—bondage to crop liens and supply merchants, inability to diversify crops or otherwise control their farming—on racism, though the same pattern then existed among white sharecroppers. At least a part of her problem is that the material about Promised Land is poorly grounded in historical context. Sharecropping, crop liens, merchant credit, and the extent of racial discriminating therein, to cite one series of examples, are discussed with no reference to the findings or interpretive issues generated by the recent scholarship of Roger L. Ransom, Richard Sutch, Joseph D. Reid, Robert Higgs, Stephan J. DeCanio, and others.

One major interpretive problem is the failure to reconcile the conflicting influences of community vitality and discrimination from the outside. Promised Land was a black community molded in no small part by segregation and discrimination. Just how much did Promised Landers benefit from community and how much did they suffer from discrimination, and how did these things interact? The evidence Bethel offers on these questions is contradictory and some of it can be read in ways she does not intend. Over and over she describes Promised Landers as independent, resourceful, confident, and proud people, albeit poor and plagued by racial discrimination. Yet, to illustrate the contradiction, she also tells us that “despite the protection from the negative effects of caste awareness the Promised Land children enjoyed, their early socialization experiences formed personalities which generally conformed to prevailing racial stereotypes” (p. 303, n. 3).

If the latter might lead one to expect a community of Sambos, Bethel’s few remarks about culture are also unexpected in view of much recent scholarship. Bethel finds at Promised Land none of the Afro-American cultural forms and influences that Eugene D. Genovese, John W. Blassingame, Lawrence W. Levine, Herbert G. Gutman, and others have found in their studies of black Americans. On the contrary, she finds the people, institutions, and values at Promised Land conventionally American, Protestant, and bourgeois. Promised Landers

seem to have had no race consciousness in the cultural sense; indeed, they did not even celebrate Emancipation Day.

I. A. NEWBY
University of Hawaii,
Manoa

WALTER L. WILLIAMS, editor. *Southeastern Indians since the Removal Era*. Athens: University of Georgia Press. 1979. Pp. xvi, 253. \$6.00.

It is an often overlooked fact in American Indian history that there remains a resident native population in the Southeast portion of the United States despite the celebrated removal policy of the Jacksonian era. Most historians may be aware that there are a few Lumbees or Cherokees in the South, but the diversity of tribal cultures would probably come as a surprise. A major reason for this oversight is that these tribal groups, often only fragments of tribes, have been either small in number, remote in location, or considered part of the black population. There is little to document their history and historians have paid scant attention to them.

This collection of essays attempts, in a very preliminary way, to bring forward the history of these groups. Although written largely by anthropologists, the essays are well-written and well-researched histories. Editor Walter L. Williams has chosen to divide the book into two sections. The first covers those native groups that avoided removal such as the North Carolina Lumbee, Louisiana Tunica, and South Carolina Catawba. These tribal groups managed to escape the removal program through sheer luck and have generally lived in the same area since the eighteenth century. The second section deals with the remnants of tribes that remarkably managed to avoid a removal program that took most of their kinfolk west to Oklahoma and Arkansas. Here are essays on such groups as the Florida Seminole and North Carolina Cherokee. These people generally lived on undesirable lands or in such inaccessible areas that their removal was either uneconomical or unnecessary.

All of the essays bring the status of these native groups up to date. Here one finds a rather wide disparity of situations. Some tribal organizations are active and recognized as official Indian tribes by the federal government. Others have no legal status and are afraid to stress their native heritage lest they create a backlash among their Southern neighbors. Part of this problem stems from the tendency of non-Indian Southerners to place the native peoples in the “colored” category. The tendency of non-Indians to lump blacks and Indians together is a major theme of the book. Another theme is that considerably more research on the Southern tribes

is necessary before we have a complete picture of the events and forces that have shaped Indian affairs in this portion of the nation.

The essays are of high quality. The authors have consulted the major archival sources and have also done considerable fieldwork. Because of the obscure nature of these tribes, there is often little information on their history during the nineteenth century, and, as a result, most of the essays concentrate on twentieth-century events. It would be nice to know more about these people in the immediate post-Removal period, but until more research is done we will have to wait.

ROBERT A. TRENNERT
Arizona State University

MARGARET WALSH. *The American Frontier Revisited*. (Studies in Economic and Social History.) Atlantic Highlands, N.J.: Humanities Press. 1981. Pp. 88. \$5.00.

Commissioned by the Economic History Society, *The American Frontier Revisited* by Margaret Walsh is a part of the series "Studies in Economic and Social History." This small study reanalyzes the impact of Frederick Jackson Turner's celebrated 1893 essay, "The Significance of the Frontier in American History." Starting with Turner's precept that the abundance of cheap lands and rich resources served as the basis for opportunity and rapid growth, the Walsh monograph focuses on Turnerian views of the economic development of the United States.

Using an analytical framework, the author selects several themes that she discusses in light of current research. To develop this study of economic development in the nineteenth century, Walsh first explains the origin and significance of the thesis, stressing Turner's interdisciplinary approach, his geographic determinism, and his rejection of the Teutonic origins of American institutions and ideas. The author then proceeds to analyze various criticisms and responses, noting that initially the thesis received little recognition due to its untraditional character and because Turner's broad generalizations failed to meet standards of scientific research. By the 1930s, however, the attack on Turner began in earnest as historians began to question whether Turner's "frontier" was an appropriate framework for the understanding of American history. Charges of ambiguity and incorrect assumptions were levied against the Turner thesis and especially against the safety-valve theory. In a fair and balanced presentation, Walsh synthesizes the views and criticisms of well-known researchers such as Ray Allen Billington and David M. Potter to conclude that the Turner thesis, if considered in the broad framework in

which it was intended, remains a viable and enduring tool for the historian of the American West.

The second and third sections of the book, which relate the thesis more specifically to American economic history, deal with the abundance of free land and resources of the frontier—that amorphous term defined alternately as a *condition* awaiting exploitation; a *process* of settlement; and as a *location* or geographic region. The author concludes this portion of her study by claiming that Turner's view of the frontier as the existence of an area of free or vacant land was erroneous but in keeping with the ethnic unawareness of the time, which lumped the native American with the environment. In similar manner, Walsh agrees with critics who rejected the idea of the frontier as a refuge for the oppressed but states, in support of Turner, that the *entire* frontier did provide economic opportunity. In analyzing the frontier as a *process* of settlement, the author proposes that geography and technology meant diversity of growth patterns as opposed to Turnerian conformity. As a geographic *location* or area of low population density, however, Walsh admits that the new research techniques based on demographic statistics and analyses are still not adequate for unqualified definition of the frontier as a region. Along these lines, in the final section, "Peopling the New Land," she points out the interesting fact that women, who constituted 40 percent of the population according to census figures, were largely unrecognized in the Turner thesis.

As Walsh indicates, although critics and disciples demand too much from the Turner thesis, this ground-breaking essay has stood the test of time. When considered as speculative rather than definitive, the thesis continues to provide guideposts for future inquiry and revision. *The American Frontier Revisited*, therefore, constitutes a concise and invaluable point of departure for the student embarking on a journey through the maze of frontier historiography.

NECAH STEWART FURMAN
University of New Mexico

WATSON PARKER. *Deadwood: The Golden Years*. Lincoln: University of Nebraska Press. 1981. Pp. xiv, 302. \$24.00.

In recent years we have been blessed with first-rate historical writing about the Western mining town and its inhabitants. Duane A. Smith has told us about towns in the Rocky Mountains, W. Turrentine Jackson about Treasure Hill, Nevada, and Ralph E. Mann, in an essay on Grass Valley, California, has shown how much may be learned by applying the new statistical techniques to manuscript census rec-

ords. Mark Wyman, Ronald C. Brown, and Richard E. Lingenfelter have taught us a vast amount about the daily life of the men and their families who lived in the mining towns.

How does this new book on the famous Black Hills mining town of Deadwood, South Dakota, measure up against these recent studies? Its author, Watson Parker, has unusual qualifications. He is simultaneously a historian and an almost lifelong resident of the Black Hills. He has a detailed knowledge of the geography, mining technology, local government, and, above all, the local people. He has spent years accumulating information, anecdotes, and legends about his town. In his preface he concedes (pp. x–xi) that “doubtless I have included in my history an untruth or two. . . . In extenuation I should point out that in many cases the lively lies and misconceptions which Deadwood’s citizens cherished were more important in shaping Deadwood’s history than truer and more sober truths would have been.”

The result is a book that is lively, colorful, and often amusing, but neither perceptive nor profound and often quite disjointed because of the author’s determination to tell still one more dubious anecdote about still one more semimythical personality. The best of the nine chapters are the first two, which describe the original rush and the types of mining it produced. Here Parker is expanding upon material covered in his earlier volume, *Gold in the Black Hills* (1966), and he gives the reader a discussion that is clear, interesting, and highly informative. Chapter 5, which takes up the change in hard-rock mining caused by the introduction of cyanidation, is less impressive, because it attempts to handle a complex subject too quickly and easily, but it is adequate. The remaining six chapters, which are primarily social history, get into trouble. In these the author allows anecdotes, myths, humor, and a surfeit of names and trivial episodes to overwhelm the main story, with the result that the book becomes not a venture into the flourishing new school of Western urban and mining town history, but rather an example of old-fashioned local storytelling. Dismissed with only a few superficial comments is the chance to contrast life in the wide-open town of Deadwood with that in Lead, three miles away, where miners lived in a big, paternalistically managed company town, as described by Joseph H. Cash in *Working the Homestake* (1973).

RODMAN W. PAUL
California Institute of Technology

EGAL FELDMAN. *The Dreyfus Affair and the American Conscience, 1895–1906*. Detroit: Wayne State University Press. 1981. Pp. ix, 187. \$17.95.

Egal Feldman’s concept for this book was novel and effective: novel because few treatments of American social history are based on perceptions of European domestic crises; effective because Feldman successfully analyzes Americans’ estimates of their legal, political, and social customs through their responses to the Dreyfus affair. Using sources of those who reflect and create opinion—the press, church publications, sermons, and collections of private papers—he reveals American ambivalence on racism, imperialism, legal procedures, militarism, church-state relations, and dangers to republicanism.

Feldman weaves events of the Dreyfus affair into his narrative and explains French reactions: antisemitism, church-army relations, patriotic reverence for the army, and monarchical-republican antagonisms. He relates American press coverage of the Zola and second Dreyfus trials and American reactions. He explains American experiences that colored those reactions: the Spanish-American War, Anglo-American legal procedures, recent Roman Catholic and Jewish immigration, and Anglo-American Protestant attitudes. “The Dreyfus affair had something to say to most Americans, lawyers and laymen, militarists and anti-militarists, reformers and reactionaries, ministers and agnostics, Protestants, Catholics, and Jews” (p. 146).

French legal procedures were criticized by lawyers, judges, the American Bar Association, and laymen who based their opinions on American press reports of the Zola and Dreyfus trials and descriptions of French appeals courts. Failing to distinguish between military and civil courts, the critics perceived Anglo-American jurisprudence to be vastly superior to the chaotic, authoritarian, “Roman-French” system. Only Irish-Americans dared question this assessment. Those same trials produced heroes for the American critics—Lieutenant Georges Picquart, Emile Zola, and his attorney, Fernand Labori—for their bravery in face of French militarism, antisemitism, and unjust French courts.

Anglo-American sentiment, however, was rejected by some Americans who saw its militaristic and imperialistic implications in the simultaneous Spanish-American War and the Fashoda diplomatic incidents. They cited the Dreyfus affair to warn against militarism in a republic.

Finally, antisemitism implicit in the Dreyfus affair revealed American social religious divisions. Protestant leaders condemned Pope Leo XIII’s silence, placing the American Roman Catholic hierarchy on the defensive. Recent Jewish immigrants reacted militantly, but assimilated ones advocated moderation. Some Protestants in their own antisemitism emphasized only the French court system’s injustice, and some Roman Catholic liberals questioned papal silence. Generally, however, America was Dreyfusard, republican, and anticlerical.

Feldman's "Coda" (chap. 7), weaker than his evidence justifies, implies that American social pluralism existed not only between social groups, but within each group.

Feldman's study leaves some questions. In Jim Crow's decade, did American blacks remain silent on French antisemitism? Did whites see no relationship between Jim Crow and Alfred Dreyfus? Furthermore, given the title dates, did Americans not react to Dreyfus's full exoneration or the Radical governments' attacks on the French army and church? Nevertheless, Feldman goes far to explain some Americans' views on Frenchmen, their customs and morality; he goes even further in unraveling the threads of American turn-of-the-century social pluralism.

WARREN F. SPENCER
University of Georgia

GERALD T. WHITE. *The United States and the Problem of Recovery after 1893*. University: University of Alabama Press. 1982. Pp. x, 160. \$13.75.

Having read the subsequent literature, the distinguished business historian, Gerald T. White, has reworked his 1938 University of California dissertation on the great depression of 1893. His argument is that the monetary and tariff debates, along with proposals for public works, crop limitation, and governmental market intervention to raise agricultural prices, pioneered the path to the general welfare state of the 1930s. "Once the belief was accepted that government could and should play a part in conquering depression," he writes in his preface, "it became possible for policy makers to consider more sophisticated arguments later on" (p. x).

Apart from statement of the thesis in opening and closing chapter paragraphs, the preface, and concluding overview, the monograph is a graceful, well-researched, and traditional presentation of the public argument over the depression and public policy. While a useful account for the general reader, the text offers little support for the larger argument. White shows that the public debate focused on currency inflation and the tariff. He clearly states that no important consideration was given to proposals for public works, crop limitation, or governmental market intervention, and he spends little time on them. The inflationary impulses of the 1890s are not substantially differentiated from those of the 1870s, and connecting links between the 1890s and 1930s are not offered.

The argument rests on a similarity of kind and a faint indication of direction: "a transition point en route toward the more positive role that the national government would be called upon to play" (p. 101).

Even if the 1890s did not see currency and tariff laws as creating an interventionist state, from a longer perspective, it was part of the movement. The question this raises is whether, even at this level, the similarity of kind is sufficient to be credited. Tariff schedules and bimetalism were congressionally prescribed, automatically functioning systems, with some presidential reciprocity and bargaining leeway. Neither foresaw continuous intervention by an enlarged "positive government." While the New Deal was concerned with both currency inflation and tariffs, both were outside the mainstream of the emerging general welfare state. At least from this reading, the concern for recovery after 1893 does not offer a viable analogue for the New Deal and the modern interventionist state.

DAVID CHALMERS
University of Florida

J. DAVID HOEVELER, JR. *James McCosh and the Scottish Intellectual Tradition: From Glasgow to Princeton*. Princeton: Princeton University Press. 1981. Pp. xiv, 374. \$25.00.

When one of his prize students returned home to Princeton after having been sent abroad to study and having been allowed to wander freely and all agog in the German universities, those dens of heterodoxy and skepticism, President McCosh insisted that the young scholar put the experience to its proper use by writing a dissertation in which the conclusions must "refute materialism." That was typical. McCosh usually wanted to have it both ways. Yet this highly intelligent and ambitious thinker and educator seemed to lack the constructive imagination to encompass the many contraries that beset him. The outcome of his various endeavors, therefore, was rarely what he had wanted or expected. In his philosophical work, he tried to hold on to the practicalism of the empiricists and the spiritual possibilities of the idealists, but his brand of realism persuaded only a few thinkers with similar Presbyterian loyalties, and even those not for very long. In his theological labors, he attempted to develop a religious outlook that would be open to notions of flux and development and yet be free of any consequent religious tentativeness or uncertainty. But he was left stranded in a religious no man's land while the opposing hosts of the godly marched off in opposite directions. As president of Princeton, he did his level best to ensure a student body of broad social backgrounds, yet at the same time he wished to maintain constantly growing enrollments. In late nineteenth-century America, however, with its rapidly widening class divisions, Princeton soon became more notably a school for rich men's sons than either Harvard or Yale, neither of which suffered

the embarrassment of democratic pretension. McCosh also championed the promotion of college athletics in the attempt to turn out cultivated gentlemen of mental agility and manly prowess. But to his dismay, college athletics quickly slid toward anti-intellectualism, professionalism, and even gambling. At least McCosh knew what the issues were.

One of the strengths of J. David Hoeveler, Jr.'s book is that it provides the basis for these somewhat adverse judgments, although the author himself presents a much more favorable but not uncritical evaluation of McCosh's accomplishments. This is a work of diligent and thorough research, with a penchant for careful description rather than vigorous analysis. Hoeveler divides his study into three parts, each depicting McCosh in a different setting—Scotland, Northern Ireland, and America; and each stressing a different aspect of McCosh's career—his upbringing and his ministerial work, his philosophical studies, and his efforts as an academic politician and reformer. McCosh's education and early career are affectionately described, and we are given to understand how easily one turns prickly amid the Scottish thistles. Hoeveler's review of McCosh's philosophical ideas is balanced and conscientious. If it is a little flat, that is because Hoeveler resists even the slightest anachronistic leavening. He does not try to make a case for the force or viability of any of McCosh's ideas, nor does he respond to the usual criticism of the Scottish intellectual tradition—that it enshrined parochial truths in the oracular categories of consciousness, intuition, and common sense. Hoeveler is at his best in the Princeton chapters. Here he sets forth a vivid account of McCosh's ups and downs and through those events provides an astute description of American academic politics in the late nineteenth century.

This is a helpful book. Those who wish to be well informed on the history of American philosophy and education would do well to read it and learn from it.

SAMUEL HABER
*University of California,
Berkeley*

MICHAEL J. DEVINE. *John W. Foster: Politics and Diplomacy in the Imperial Era, 1873–1917*. Athens: Ohio University Press. 1981. Pp. x, 187. \$15.50.

John Watson Foster, who served briefly as secretary of state during the waning months of President Benjamin Harrison's term, is known to present-day historians mainly because he was Robert Lansing's father-in-law and the grandfather of Allen, Eleanor, and John Foster Dulles. Michael J. Devine has assumed the task of providing the first comprehensive appraisal of Foster, a task made difficult because Foster destroyed his personal papers after prepar-

ing his memoirs. Devine relies on official records, his subject's extensive publications, and traces found in the records of his correspondents.

Foster is of interest today because he was a prototype of the private citizen called regularly to government for important assignments. Of special note are his diplomatic services in Mexico, Russia, Spain, and the Far East. The most important episode of his short stay at the Department of State was an abortive attempt to annex the Hawaiian Islands, a subject that Devine examines carefully in a detailed chapter. Foster's performance in this matter reflects his support of the imperialist movement that waxed and waned during the 1890s and after.

Between periods of official service Foster pursued his principal vocation, the practice of international law, and managed to accumulate considerable personal wealth. In this connection he represented a number of foreign governments, but he was apparently unconcerned about what today might be considered serious conflicts of interest. Foster's legal career led him, as seems to have been common at the time, to an extensive interest in the pre-World War I movements for peace. Devine provides some useful information on this subject.

Devine does his best to maintain objectivity. Like many students of late-nineteenth-century figures, he shows sympathy for his subject but also notable concern about various episodes—understandable in the moral and intellectual context of the age but hard to justify in today's climate.

Historians now have at least a reasonable facsimile of Foster the public man. This modest but worthwhile contribution rescues a second-line figure from distortions and omissions in his record as a lawyer and government servant. Historians will make good use of this work as they move toward a modern synthesis of the years between 1865 and 1917. Devine demonstrates once again the utility of sound information about the historical actors in support of the great, those behind the first echelon.

DAVID F. TRASK
U.S. Army Center of Military History

DAVID MCCULLOUGH. *Mornings on Horseback*. New York: Simon and Schuster. 1981. Pp. 445. \$17.95.

Theodore Roosevelt has always been an attractive subject for biographers, and given his colorful personality and the active life he led it is not surprising that this is so. But unlike most American presidents, Roosevelt's life before he became a national figure has been the subject of at least as much popular and scholarly interest as his presidential career. Since 1958 there have been three solidly researched books dealing with all or part of his early life: Carlton Putnam's *Theodore Roosevelt: The Formative Years*,

1858–1886; Edmund Morris's *The Rise of Theodore Roosevelt*; and the volume under review, David McCullough's *Mornings on Horseback*, which ends with Roosevelt's second marriage in 1886. McCullough's book is, in my opinion, clearly the best, a work of art as well as an excellent piece of scholarship.

Interest in detailed study of the "early" Roosevelt is partly due to the fact that he and the members of his family have left us masses of revealing letters, diaries, and memoirs. They were a close-knit and extravagantly outgoing group of people, interesting as individuals and fascinating in their family relationships. More than any of his predecessors, McCullough has taken the entire family as his subject; TR stands in the center of the group portrait, but his parents and his siblings are drawn to scale. The author's treatment of Theodore Senior is masterful. We see the paterfamilias who makes the rules, doles out the spending money, shields all from the buffets of fate and the outside world, yet who also encourages and at times requires all to stand firmly on their own. Here too is the public-spirited citizen with his committees and his charitable work and also the lion of New York society and the free-spending Gilded Age millionaire. (Only a full treatment of Roosevelt's business activities is missing.)

The picture of Martha Bulloch Roosevelt is less satisfactory. It is not so much that McCullough explains away her eccentricities (although that he certainly does) but rather that he fails to make her come alive the way all the others do. The treatment of TR's older sister Anna (Bamie) is throughout brilliant—she emerges as the cement holding the family together and in her way an even more interesting person than Theodore.

Of course in a book of this sort what matters most for historians is what light it sheds on the young Theodore Roosevelt. McCullough avoids both broad generalizations and psychological theories, preferring to describe what Roosevelt said and did and let readers form their own conclusions. Yet the work is full of shrewd judgments, and there are some remarkable insights. He is the first biographer to notice that a preponderance of Roosevelt's asthma attacks occurred on Sundays, when his beloved father was available to nurse him. He is not the first to note Roosevelt's enormous energy, but he stresses the enormous variety of his activities and their incessant quality. Roosevelt, as he puts it, was "a bee in a bottle" (p. 206).

All the well-known Roosevelt stories are here—the French dueling pistols, the captured thieves in the Bad Lands, the adventures in the New York assembly. Yet McCullough usually has something fresh to say even when dealing with the familiar. He points out that Roosevelt's family and most of his

society friends heartily approved of his entry into the political arena. He shows that despite his fascination with ranching in the Bad Lands, TR never remained away from New York City for more than a few months at a time, and he adds: "most of his best writing about his home on the range was written at home in the East" (p. 337).

In 1875 Theodore Senior referred to his namesake as "a noble boy" (p. 147), and five years later Alice Lee, his betrothed, spoke of him as "a noble man" (p. 224). The qualities they noted are easy to caricature, as Henry Pringle demonstrated many years ago. Somehow McCullough makes Roosevelt's "nobility" both believable and attractive. He shows the self-righteousness, the stuffiness, and the compulsiveness, but also Roosevelt's tenderness, his loving nature, his unique combination of unswerving devotion to the canons of conventional morality and lack of the crabbed characteristics that often accompany such devotion.

It would be foolish to think that the last word has now been written about the formative years of Theodore Roosevelt's life. But those who seek to improve on David McCullough's account will have their work cut out for them.

JOHN A. GARRATY
Columbia University

DAVID E. SHI. *Matthew Josephson, Bourgeois Bohemian*. New Haven: Yale University Press. 1981. Pp. xiii, 314. \$19.95.

In many ways the career of Matthew Josephson encapsulated the history of literary radicalism in the United States from the First World War through the McCarthy era. In a book that focuses on the enigmatic relationship of art to society and the accompanying problem of the artist as citizen and public man, David E. Shi has written an admirable intellectual biography that not only succeeds in sketching the life and thought of Josephson but also brilliantly describes the larger cultural milieu of his times.

While still an undergraduate at Columbia, Josephson became a part of the literary avant-garde in Greenwich Village and was an intimate of Malcolm Cowley, Kenneth Burke, Hart Crane, and William Slater Brown, among others. Always a spirited and contentious polemicist, Josephson articulated an aesthetic extremism that first became visible in his championing of the Dada movement but would also be manifest in his early and uncritical enthusiasm for the machine age, in his excoriation of the expatriates, and in his combative pamphleteering. Shi believes that the transformation of Josephson from literary to political radical was a natural outgrowth of his temperament, a transition that was abetted by his brief career as a

customer's man on Wall Street, his research for a biography of Emile Zola, a nearly fatal fire that occasioned a lengthy hospitalization, and the Great Crash of 1929. Throughout the 1930s he was a prominent fellow traveler although he never became a Communist party member.

Josephson never realized his ambition to become a modernist poet or successful novelist, but he found his *metier* in biography and history. In addition to his study of Zola he wrote vivid biographies of Rousseau, Hugo, and Stendahl. Perhaps his most enduring work was a historical trilogy outlining American history during the Gilded Age. Informed by an economic determinism derived from both Karl Marx and Charles Beard, *Robber Barons*, *The Politicos*, and *The President-Makers* were widely read and especially influential among academic historians.

Shi is best in uncovering and explaining the ambiguities in Josephson. He contrasts his social activism with his bourgeois lifestyle, his advocacy of a collectivist society with his hatred of regimentation, and his domineering personality with his innate kindness. The author also has skillfully blended Josephson the man, husband, and father with Josephson the man of letters. But Shi has produced more than a narrow monograph; he has illumined a critical and volatile period in the recent past that contributes much to our understanding of both the literary left and its relationship to the nation's politics and intellectual currents. Josephson was embroiled in the great controversies that so agitated America's intellectuals from 1920 to 1950: the nature of Stalinist Russia, the efficacy of Roosevelt's New Deal, the meaning of the Nazi-Soviet Pact, the Henry Wallace candidacy in 1948, and the trial of Alger Hiss. In tracing Josephson's career Shi has told us much about those turbulent years. This is not only a good book; it is an important one.

TIMOTHY P. DONOVAN
University of Arkansas,
Fayetteville

HOWARD W. ALLEN. *Poindexter of Washington: A Study in Progressive Politics*. Carbondale: Southern Illinois University Press. 1981. Pp. xiv, 334.

Howard W. Allen's biography of Miles Poindexter illuminates several important themes: the rise and fall of political Progressivism, the regional characteristics of much of the reform impulse, and the shift from Progressive reforms to the Red Scare. The book is a balanced, interpretive assessment that carefully places Poindexter within the context of national Progressive politics.

Poindexter, a middle-class lawyer and member of Spokane's business-professional elite, served in the

House from 1909 to 1913, and then in the Senate until 1923. He was a leading Republican insurgent and an ardent supporter of Woodrow Wilson's "New Freedom" legislation. Allen analyzes congressional votes to show both Poindexter's strong reform record and the regional influences on it. Drawing on Samuel Hays's insights, Allen suggests that most congressional insurgents "were reformers mainly because they were defending the interests of their section of the country against eastern 'colonialism'" (p. 166). The point is particularly helpful for understanding Poindexter's early maverick political tendencies. It is less useful regarding his conduct by 1917. As Allen recognizes, not all Western insurgents lined up with Poindexter during the war and Red Scare. Ultimately, Poindexter most closely resembled his hero Theodore Roosevelt, that urban Easterner with only tentative connections to Dakota country.

In 1912, despite some initial skepticism, Poindexter plunged enthusiastically into the Bull Moose movement. Indeed, he hoped to be a future Progressive presidential candidate. By 1914 he was unquestionably one of the more advanced national political reformers. He had defended the IWW, advocated a large public works program for the unemployed, sponsored a bill to protect Alaskan resources from corporate development, delayed Woodrow Wilson's inaugural ceremonies with a long Senate speech on women's rights, and fulminated against economic trusts.

Then, abruptly, Poindexter veered sharply to the right. Always a zealous exponent of American military power, he called for war against Mexico and privately endorsed annexing the northern part of that nation. Like Roosevelt, he was anxious for America to enter the Great War. His increasingly strident 100 percent Americanism characterized wartime intolerance and the Red Scare at their most extreme. By 1920 he was firmly in the Old Guard Republican camp, defending big business and attacking labor. His conservatism understandably angered western Washington's working-class voters, who helped to defeat him in 1922. Warren Harding appointed him ambassador to Peru, but in 1928, after losing another bid for the Senate, Poindexter retreated from public life.

Allen makes sense of Poindexter's zigzag career. In fact, he argues convincingly that Poindexter was remarkably consistent. Like many Progressives, Poindexter crusaded for righteousness and pursued enemies of the people. Allen shows that Poindexter at first saw big, eastern corporations as most threatening to the public. By 1917, however, he had concluded that the real danger came from the left, swelling upwards from labor unions and new immigrants. Predisposed to battle "special interests," Poindexter redefined the nature of the enemy. He

was not the only reformer to do so. One result was that important aspects of Progressivism degenerated into the Babbitry of the twenties. This book, well researched and thoughtful (although somewhat mechanical and uninspired stylistically), helps to explain why this occurred.

LEROY ASHBY
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PAUL D. CASDORPH. *Republicans, Negroes, and Progressives in the South, 1912-1916*. University: University of Alabama Press. 1981. Pp. ix, 262. \$18.75.

During his long public career, Theodore Roosevelt confronted again and again the issue of establishing a viable political base in the solidly Democratic South. As president he alternately employed black Republicans, their lily-white counterparts, and Southern Democrats to strengthen the GOP in Dixie. When he led the Progressive party between 1912 and 1916, Roosevelt excluded blacks from party deliberations in favor of "the best white men in the South" (p. 146). This latter phase of Roosevelt's political relations with the South forms the subject of Paul D. Casdorph's interesting review of Republican-Progressive warfare from 1912 onward.

Casdorph constructs his narrative from extensive research in Southern newspapers and a cross section of the manuscripts of those involved. He might have looked at the papers of William Howard Taft's campaign manager, William B. McKinley, at the George Arents Library, at Syracuse University, and the collections of Robert M. LaFollette, Gifford Pinchot, James R. Garfield, and others at the Library of Congress. Some useful documents and articles on the 1912 Republican national convention also escaped his attention. There are factual slips as well in Casdorph's treatment of the national scene. For example, he places the launching of Taft's renomination campaign at least six months later than it actually began.

For the most part the book reviews state-by-state the internecine struggle among black and white Republicans in the South. Readers will find his matter-of-fact approach makes for slow going. The biographical sketches that are provided lack bite and insight, and Casdorph reaches no striking analytic conclusions. He argues that these events may have contributed to eventual black disenchantment with the GOP in the 1930s but recognizes that such effects came slowly until the New Deal. He is hard on Taft's national record but shows that the president fought adroitly in the race for the 1912 nomination against LaFollette and Roosevelt. More attention to Charles D. Hilles, Taft's secretary, whose career was not "checkered" (p. 26), would have brought a shrewd political operator into clearer

focus. Casdorph's treatment of the Republican national convention in 1912 illustrates the need for a detailed examination of that important turning point in Republican party history.

The book does not confront directly the question of Theodore Roosevelt's skill as a party leader. To what extent did the future president base his decision about the South on information from regional operatives who told him just what he wanted to hear? How well did Roosevelt appraise the capacity of those who followed him into the Progressive party? The improvised Southern strategy that Roosevelt pursued in 1912 over the contested delegations led inexorably to the cries of theft that laid the basis for the Progressive breakaway. These issues do not arise often enough in this study, and it is most useful as a compendium of information about a tawdry and peripheral chapter in the politics of the Progressive era.

LEWIS L. GOULD
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DAVID M. KENNEDY. *Over Here: The First World War and American Society*. New York: Oxford University Press. 1980. Pp. vii, 404. \$19.95.

The origin and the fighting of war have always attracted more attention among historians than the impact of the struggle on society, but since our nation's time of troubles during the Vietnam conflict we have seen a real growth of scholarly interest in what the process of war does behind the battle lines. Here now we have—somewhat later than might have been expected—the first contemporary synthesis of work accomplished with regard to America's internal experience in World War I, a suitable companion to the fine studies of Richard Polenberg and John Blum on the home front in the Second World War and Alexander Kendrick and Godfrey Hodgson on the domestic side of the Vietnam struggle. This is an impressive effort by a historian clearly familiar with the wider context of the First World War and particularly sensitive to the relationship of the conflict to the Progressive reform movement that preceded it. The war, David M. Kennedy says, provoked the nation as never before to organize itself efficiently, but he also suggests that one can exaggerate the modernity of America's response. Indeed, Kennedy believes that, by occurring when it did, at the end of an era of "peculiarly intense disagreement about the principles of political economy" (p. 137), the war fostered an idiosyncratic, ambiguous, and potentially reactionary form of mobilization featuring "massive informal collusion between government and organized private enterprise" (p. 141). This overlapping of the two

sectors, which was to recur here in later periods of crisis, was the direct outgrowth of the Wilsonians' (and Americans' continuing) unwillingness to exercise power in any formal, straightforward, or forceful way—of their phobic refusal to carry out the rationing, taxing, requisitioning, and coercing that European governments had long since undertaken. Such a renunciation of authority had a second unfortunate effect as well, for it led of necessity to the Wilson administration's deliberate propagandizing and agitation of the public as a means of achieving needed discipline, whipping up a patriotic hysteria that would ultimately undermine progressive forces politically and help to undo the benefits that groups like workers, blacks, and women derived from the war. Yet Kennedy recognizes that on occasion the conflict also worked to the advantage of America and Americans, as, for example, in creating large surpluses in our international balance of payments, surpluses, he points out, that in the 1920s we did not use as constructively as Britain had employed its surpluses before 1914.

If Kennedy's work on the consequences of war is not as comparative as Arthur Marwick's or as precise in measurement as Arthur Stein's, it is nevertheless a very careful and thoughtful study, fascinating in its insight, panoramic in its sweep.

KEITH L. NELSON
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CARL H. CHRISLOCK. *Ethnicity Challenged: The Upper Midwest Norwegian-American Experience in World War I*. (Topical Studies, number 3.) Northfield, Minn.: Norwegian-American Historical Association. 1981. Pp. 174. \$10.00.

The best treatment of the Norwegian language in America is Einar Haugen's definitive study of 1953, with chapters (11 and 12) entitled "The Struggle over Norwegian" and "The Triumph of English." In Carl H. Chrislock's new book, the struggle over Norwegian is also central but limited to its role in the clash between ethnic preservation and antihyphenism during the years 1915–22. The first chapter, entitled "1914" (pp. 7–28), describes, on the one hand, the sentiments of prosperous Norwegian-Americans visiting the old country in the year of its centennial celebration of nationhood and, on the other hand, analyzes the state of Norwegian institutions in America—the Norwegian press, church, social clubs, literary societies, and so forth. Two basic attitudes are indicated and exemplified by Luth Jaeger's nativism and Valdemar Ager's cultural pluralism. The conflict between the two attitudes is taken up in chapter 2, "The Hyphen Must Go!" (pp. 28–55), which asks to what extent

prohyphenism was at the same time pro-Germanism, discusses the effects of President Wilson's Philadelphia address in May of 1915 to an assembly of recently naturalized immigrants ("You cannot become thorough Americans if you think of yourselves in groups"), and comments on the congressional vote in April of 1917 to join the Allies in their war against Germany: 455 to 56 for, with Norwegian senators and congressmen voting 6 to 4 against.

Chapter 3, "Making the World Safe for Democracy" (pp. 56–88), is concerned with the activities of the Minnesota Commission for Public Safety, its support of alien registration and the demand for translation and public scrutiny of all articles in the Norwegian-language press dealing with government policy. The climax of the war hysteria is seen in Governor Harding's (Iowa) proclamation of May 23, 1918, prohibiting the use of languages other than English in schools, public places, on trains, and over the telephone. Party politics is discussed in chapter 4 ("The Politics of Loyalty" [pp. 89–121]), dealing mainly with the futile attempt of the Non-partisan League based in North Dakota to extend its power into Minnesota in 1918, and with a similar setback for the Norwegian progressives in Wisconsin. The fact that radical politics in the upper Midwest gained new strength in the 1920s is seen here to be the result not only of Coolidge and Harding's economic policies but also of the excesses of the loyalty crusade. In a final chapter, "The Aftermath" (pp. 122–44), Chrislock shows that, while the continued crusade in the early postwar years against foreign languages did not result in prohibitive legislation, there was among the young a waning interest in bilingualism. Chrislock discusses the decline of the old microethnic societies called *bygdslag* as well as the decline of a newly founded literary society *For Faedrearven* and concludes his argument by deploring the "Americanization craze," which precipitated a state of affairs where "ignorance of Norwegian was a mark of distinction and a Scandinavian accent a serious handicap" (p. 144).

Ethnicity Challenged is a useful book on a perennial problem in modern democracy—how to safeguard and profit from minority opinion. It combines Haugen's account of the Norwegian language struggle and Jon Wefald's analyses of Norwegian-American politics (*A Voice of Protest* [1971]) with Chrislock's own treatment of such hyphenists as Valdemar Ager (in *Cultural Pluralism* versus *Assimilation* [1977]). Of particular interest is his interpretation of the Knute Nelson–Nicolay Grevstad relationship. Admittedly, the author confines himself to the Norwegian story—and to nonquantitative methods in a field well suited for statistical research; there are no illustrative comparisons with statistical data from other Scandinavian immigrant groups. Even so,

those among future scholars who wish to create a more concentrated and more comprehensive picture of Scandinavian-American nativism during and after World War I will find Chrislock's book with its very full documentation to be a source of considerable value.

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JOSEPH R. CONLIN, editor. *At the Point of Production: The Local History of the I.W.W.* (Contributions in Labor History, number 10.) Westport, Conn.: Greenwood Press. 1981. Pp. viii, 329. \$29.95.

At the Point of Production is a three-pronged attempt to spur interest in the activities of the Industrial Workers of the World at the local level. One part is Joseph R. Conlin's introductory survey of a half-century of writing about the IWW, another is Dione Miles's eighty-page bibliography, and a third is the compilation of ten essays, each by a different scholar, treating little-known local chapters of IWW history. The reason for focusing on the IWW at the grass roots, explains Conlin, is that Melvyn Dubofsky's monumental 1969 work, *We Shall Be All*, closed the general subject of the IWW for a while, and at the same time new methodologies and approaches to labor history encouraged local studies.

The essays forming the heart of *At the Point of Production* reveal the variety of work that has already been done. Grouped into three categories treating the IWW in the industrial cities, on the extractive fringe, and "after the fall," the ten essays detail Wobbly activity among textile operatives in Little Falls, New York, and Paterson, New Jersey, rubber workers in Akron, "stogie" makers in Pittsburgh, lumber workers in eastern Texas and western Louisiana, harvest hands of the Great Plains and Washington's Yakima Valley, Colorado coal miners, and construction workers on Boulder Dam. Though versions of many of the pieces appeared earlier in state and regional history journals, it is convenient to have them in a single source.

Unfortunately, although this potpourri extends our factual knowledge of the IWW, it provides no new interpretive framework to guide the local studies it seeks to encourage. As Conlin remarks in his introductory essay, "The immediate effect of 'little' episodes involving the Wobblies will be, collectively, to bewilder. What Brissenden and Dubofsky made coherent will become confusing and irreconcilable" (p. 24).

One impression that does emerge from the "little" studies is how often Wobblies, especially the organizers, were perceived as outsiders in a community and thus became easy targets for repression by local

elites determined to protect their social and economic hegemony. Repression by local authorities emerges as a chief reason for Wobbly failure. Collectively, these studies are a tribute to the courage of Wobbly organizers who ventured into hostile locales and the workers who dared to join the union.

For many readers the book's most valuable section will be Dione Miles's lengthy list of sources for the local history of the IWW, which includes not only books and articles about the Wobblies but also IWW pamphlets and newspapers, archival holdings, state and federal documents, and theses and dissertations. Miles's bibliography would by itself justify the purchase of this volume.

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STEPHEN MEYER III. *The Five Dollar Day: Labor Management and Social Control in the Ford Motor Company, 1908-1921.* (SUNY Series in American Social History.) Albany: State University of New York Press. 1981. Pp. x, 249. Cloth \$34.00, paper \$9.95.

Historian Stephen Meyer III notes that when the Ford Motor Company introduced its Five Dollar Day profit-sharing plan in 1914, it became "the most famous labor-management reform in the annals of American business" (p. 108). Using Ford's Highland Park factory as his model, the author analyzes the origins, operations, and eventual failure of this experiment in Progressive era welfare capitalism. He considers it an attempt by management to overcome labor problems created by Ford's modern industrial technology.

Ford engineers, through a series of brilliant innovations, completely revolutionized automobile making, but their changes also produced costly social consequences. Technology had eliminated the multiskilled, relatively autonomous craftsman and replaced him with a de-skilled specialist who performed only one task all day long. While outsiders marveled at the efficiency of the new system, laborers complained of its monotony and numbing effects. Absenteeism, "soldiering," restriction of output, the slowdown, and chronic turnover plagued Highland Park. In addition, the masses of new Southern and Eastern European peasant immigrants who now staffed Ford plants brought pre-industrial attitudes and work habits with them. Language barriers and ethnic tensions further exacerbated the problem. Despite remarkable technological advances, production of individual Ford workers fell below expectation.

In order to overcome their labor difficulties, Ford executives introduced the Five Dollar Day profit-sharing plan. All workers drew wages but only those who met Ford standards received profit that repre-

sented the difference between standard pay and five dollars per day. Laborers qualified only if they exhibited sobriety, thrift, good personal habits, and a wholesome home life. Ford's newly established Sociological Department dispatched investigators to every worker's home; they checked into each laborer's marital relationship, lifestyle, savings, loans, mortgages, and debts. Ford schools taught English and conducted Americanization programs to inculcate middle-class values and customs. As a result, production increased, labor turnover declined, and Ford employees fattened their bank accounts. Nevertheless, company laborers intensely resented the Sociological Department's constant prying, and the Americanization efforts never really eradicated European nationalist sentiments, radical thinking, or prounion ideas. During the recession of 1920–21, faced with serious economic difficulties, Ford jettisoned profit sharing, disbanded the Sociological Department, cut the workforce, doubled Model T production, and granted a relatively small wage increase. Ford's scheme to improve production by exerting social control over the workers failed in part, Meyer concludes, because employees accepted its paternalistic indignities only so long as it paid off—lack of payment meant the end of docility.

Meyer's study exemplifies the "new" labor history that emphasizes the worker rather than his institutions. With its attention to class, ethnicity, and work processes, his book resembles the contributions of Herbert G. Gutman, David Brody, and David Montgomery. Yet it goes beyond the history "from the bottom up" approach, for Meyer also shows how management decisions deeply affected workers' lives. His well-written and well-researched study persuasively demonstrates that labor history must also be viewed "from the top down."

GRAHAM ADAMS, JR.
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P. K. EDWARDS. *Strikes in the United States, 1881–1974*. New York: St. Martin's Press. 1981. Pp. xvi, 336. \$27.50.

This book originates in Warwick University's industrial relations division and is one of the titles of its publication program. If it is a source of perplexity to some that a macroanalysis of American strikes comes to us from England rather than from one of our own industrial institutes, it is probably to be explained by the fact that so many American labor economists have been more interested in recent decades in probing the more orderly relations generated by a mature unionism functioning within an advanced industrialism than in trying to establish patterns in massive strike statistics. P. K. Edwards, reared in a different environment, is convinced that there are clear patterns, and that they can be traced;

hence, the focus of his study is on general trends relying on the statistics for underpinning of his theses.

Employing linear techniques, as well as regression analysis, he develops tables of index figures embracing the ninety-three years with the mass of data ranging from duration of strikes to correlations between strike indexes and different variables. For me, this is the most useful part of the book. It furnishes the student with an array of information to which reference can be made and against which conclusions can be checked in the broadest reaches of sociological investigations.

Of course, statistical studies, all the more so when involving figures over different historical periods and a variety of categories, have to be handled with great circumspection. For certain years, particularly the earlier ones, some data is sketchy, unreliable, or not available. The earlier system of recording by the Commissioner of Labor changed after the establishment of the Bureau of Labor Statistics, and the Bureau itself introduced redefinitions and innovations on three different occasions. Moreover, quantitative measurements have severe limitations, and may suggest, at times, false conclusions. For example, the United Auto Workers Union conducted strikes against General Motors in both 1937 and 1970. The bare statistics—number of plants struck, number of workers involved, number of strike days lost—all indicate that the latter was the banner strike piling up a record for intensity and depth in labor-management conflict. Actually, the 1937 strike was a small-scale civil war on the outcome of which hung the fate of the union and the future of employer-labor relations in the industry. The 1970 strike was a stylized, symbolic engagement, the rough results of which could be anticipated by both contestants, and in the course of which the company agreed to advance the money to the union so that it could meet the \$23 million a month insurance payments due for the GM membership. After the strike was over, the UAW repaid the \$46 million loan at 5 percent interest. As Walter Reuther quipped in connection with another strike, "Karl Marx would never believe this."

The author is well aware that aggregate statistical series cannot stand independently of empirical investigations. But his entirely admirable efforts to explicate the figures and equations are not always successful. In his zeal for patterns he sometimes comes up with genuine insights; at other times, his zeal results in explanations that are diffuse, rambling, or esoteric. Even this reader who took copious notes as he went along found himself tossed hither and yon by a dozen different eddies and whirlpools without reaching any dominant currents. Other theorists who have labored in these waters have encountered their full share of difficulties.

The refractoriness of the materials is illustrated

by the riddle that Edwards seeks to unravel in the concluding pages of his book. The well-known paradox is this: Why does the American working class, markedly conservative in its political and social behavior, have the highest and longest strike record in the industrialized world? Edwards's explanation (along with random, passing, side ruminations) is that the intensity of the American strike movement derives from an innate struggle for shop control, and that the past tradition of violence and employer recalcitrance has made the tendency for long strikes self-perpetuating. This is a very poor explanation in a field where none of the explanations are conclusive. Space considerations forbid me from expatiating on the matter. I only bring it up to convey the idea that Edwards in his book gives resonance and intelligent consideration to numerous important labor problems, and that time spent on it will be found rewarding.

BERT COCHRAN

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MICHAEL P. MALONE. *The Battle for Butte: Mining and Politics on the Northern Frontier, 1864–1906*. (Emil and Kathleen Sick Lecture-Book Series in Western History and Biography, number 4.) Seattle: University of Washington Press. 1981. Pp. xiv, 281. \$17.95.

RICHARD D. LUNT. *Law and Order vs The Miners: West Virginia, 1907–1933*. Hamden, Conn.: Shoe String Press. 1979. Pp. 223. \$19.50.

These books traverse familiar territory. Michael P. Malone's superbly crafted narrative treats the political economy of Butte and Montana from the gold boom of the 1870s through the absorption of the copper independents by the Standard Oil-financed Amalgamated Copper Company in the first decade of the twentieth century. Richard D. Lunt's somewhat formless and disjointed account focuses on the use of the injunction and physical force to establish "law and order" in the coal fields of West Virginia.

Lunt has a special appreciation for the ironic and a sure command of the primary sources, and the combination makes the book entertaining, if not lucid. At one point, for example, he describes a coal association employment agent defending the use of strikebreakers on the grounds that "we are fighting them on scientific methods, by using sociological facts. We understand labor is nothing but a commodity, and it is governed, not by trades organizations, but by supply and demand" (p. 28). Take U.S. Circuit Court Judge Alston G. Dayton, who rationalized his predictable anti-UMWA decisions as the product of periods of study so stressful as to bring on fainting spells. Lunt captures West Virginia Governor Ephraim F. Morgan, only days after federal troops had suppressed an actual shooting war

in Logan County, characterizing labor-management relations in unorganized counties as "amicable" (p. 124). And he senses the comedy and pathos inherent in the clumsy efforts of the Billy Mitchell-led Army Air Force to play a role in ending the 1921 disturbance. "The bombs," Lunt adds parenthetically, "went by railway express" (p. 139).

Unfortunately, Lunt's stories and quotations are set within no especially compelling or revealing framework. The reader is not prepared to understand why, for example, the politics of injunction became illegal in 1932. The idea that political change is premised on a deeper "consensus" is offered to explain the popularity of Norris-LaGuardia, but Lunt provides very little evidence to demonstrate the existence of such a consensus.

Malone's informative book is about Butte, the business of copper mining, entrepreneurship, and especially the development of the state's peculiarly vicious style of factional politics. But it is also a moral odyssey, a search for something redeeming in the ethical cesspool of Montana in the Gilded Age. It is a frustrating search. Although Malone believes that the requirements of order and efficiency made the "unification of ownership and management" imperative (and thus the battle for Butte inevitable), he has no fondness for giant, impersonal corporations like Amalgamated Copper, which "bestrode the state like a great, insensate colossus" (p. 190). Malone might have embraced the "great captains of frontier industry" (p. 157), Horatio Alger figures Marcus Daly, William A. Clark, and F. Augustus Heinze. But he finds little to value in Clark, whose efforts to secure fame and political power are chronicled here in gruesome detail (his agent in these matters reportedly said, "I never bought a man who wasn't for sale" [p. 113]). Daly was less grasping and more down to earth, yet he was also willing to engage in the politics of bribery. Malone is drawn to Heinze's last stand against the Amalgamated in the courts. But these efforts, as Malone acknowledges, depended on Heinze's ability to corrupt the courts, and Heinze's attempt to defeat the Amalgamated under the banner of democratic populism and to pose as the "hometown champion" (p. 53) was little more than a ruse.

Grasping for straws, Malone waxes eloquent over a minor character in these dramas, state senator Fred Whiteside, who testified to Clark's malfeasance, and he creates a kind of mythology of community around the independent operations of Clark, Daly, and Heinze. According to this mythology, Butte was "an unassuming democracy," where very different kinds of people coexisted in an atmosphere of high individualism and "unquestioning tolerance of one's fellow man . . ." (p. 71). Themselves risen from the ranks of miners, owners had "empathy" with their employees and treated them well. Behind the "ugly political culture" (p. 123) that

was the legacy of the copper kings was an oddly benevolent economic order. This bias—toward the mining camp and the spirit of entrepreneurial capitalism—may well account for Malone's inability to explain Montana's extraordinary political factionalism.

Nonetheless, *The Battle for Butte* is fine history: rich in detail, full of finely drawn people, masterfully clear where the subject matter is most complex, constructed to preserve something of the tone and atmosphere of the age. Unlike Lunt's account, which finds comfort in national politics (the last chapter is titled "Reform"), Malone's is a profoundly cynical retelling. And rightfully so, for if the battle for Butte was inevitable, it mattered little who won. That, in a nutshell, is the tragedy of American politics.

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HARVEY A. LEVENSTEIN. *Communism, Anticommunism, and the CIO*. (Contributions in American History, number 91.) Westport, Conn.: Greenwood Press. 1981. Pp. xii, 364. \$29.95.

This meticulously researched and informative book deserves a prominent place on the lengthening shelf of revisionist works that are now providing us with a much more fair-minded and realistic appraisal of the influence of communism in the American labor movement of the 1930s and 1940s than was available ten years ago. Rejecting both the virulently anti-Communist interpretations of 1950s writers such as Max Kampelman, as well as the somewhat self-serving accounts of W. Z. Foster and other participating leftists, Harvey A. Levenstein advances further down the path opened up by Roger Keeran in his pioneering *The Communist Party and the Auto Workers Unions* (1980). Unlike Keeran, Levenstein deals with the whole range of unions that were either led or strongly influenced by Communists during the formative years of the CIO—ranging from the relatively small Fur and Leather Workers Union centered in New York, to such giants as James Carey's United Electrical and Radio Workers of America, and Harry Bridges's International Longshoremen's and Warehousemen's Union on the West Coast—that at their peak encompassed approximately one million workers, or almost one-third of the entire membership of the organization.

This broader scope, however, also enables Levenstein to expand upon Keeran's earlier conclusions concerning both the organizational achievements of the Communists, and the errors of judgment that, along with the repressive measures of the Cold War, prevented them from being more suc-

cessful than they were. Among the achievements, the author amply documents the sincerity, devotion, and skill that the Communists and their sympathizers brought to the task of enrolling workers into unions; their unique contribution in helping to advance the cause of civil rights and of black participation in the labor movement; and the flexibility, rather than rigidity, that they displayed in enacting Communist party policy directives on the domestic front. Among the errors, Levenstein effectively analyzes the pusillanimity of the party leaders in failing to press their beliefs as radicals during the Popular Front period; the damage done to the Communist cause by their support for the no-strike pledge during World War II; and the disastrous consequences of the party's endorsement of third-party candidate Henry A. Wallace in 1948, largely on foreign policy grounds. This act appeared treasonous to Philip Murray and other CIO liberals, because in abandoning President Truman and the Democrats the Communists appeared to be deliberately running the risk of precipitating the election of an antiunion, Republican administration instead.

None of these conclusions is particularly original, or startling. Nor will nonspecialists find Levenstein's labyrinthine and somewhat narrowly conceived account of the internal politics of his chosen unions easy reading, much less entertaining. Nevertheless, besides documenting these developments more clearly than ever before, on at least one major issue Levenstein does break important new ground. This is to show that, contrary to the general assumption that the 1930s was a period of general tolerance toward radical beliefs, efforts to undermine the growth of Communist power in the labor movement were already being undertaken by both government and CIO leaders like Philip Murray before the Second World War, rather than after it. In one sense, this shows that Communist influence in the American labor movement was always more precarious than some observers have led us to believe. Yet in another, it renders their achievement in reaching large numbers of American workers during that critical decade all the greater.

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KENNETH PAUL JONES, editor. *U.S. Diplomats in Europe, 1919–1941*. Foreword by ALEXANDER DECONDE. Santa Barbara, Calif.: ABC-CLIO. 1981. Pp. xxiii, 240. \$35.00.

This volume consists of ten original essays on prominent Americans involved in European affairs in the interwar era. Indeed, among the many strengths of this volume is the broad definition of "diplomats" to

include political appointees like Joseph P. Kennedy, Alanson Houghton, and Claude Bowers, professional foreign service officers like Hugh Gibson and Loy Henderson, and unofficial economic statesmen like Thomas W. Lamont and Owen D. Young. All the essays in the volume are based on extensive research in archival resources or manuscript collections. All the essays are lucidly written and coherently organized. After very brief biographical sketches, the authors focus on one or two major aspects of the careers of these "diplomats" and try to draw some conclusions about the overall nature of American foreign policy. The result is that the essays serve as a fine introduction to the diplomacy of the era as well as to some of the most recent scholarship.

The essays on the 1920s illuminate the active nature of American diplomacy during the Republican ascendancy and illustrate the substantial financial leverage that the United States exercised in European affairs in the initial postwar decade. Michael J. Hogan's penetrating account of Thomas Lamont's approach to the reparations controversy highlights the influence of American bankers. More importantly, Hogan outlines in a brief but sophisticated manner the "corporatist" nature of American attempts to manage the international economy. This involved an "emphasis on private leadership and expertise, cooperation among functional groups, and voluntary public-private power-sharing" as well as a "cooperative multinationalism" (pp. 7, 20).

The other essays do not live up to the exceptionally high standards set by Hogan. Nevertheless, Kenneth Paul Jones's incisive analysis of Houghton's role in the Ruhr crisis, John Carroll's assessment of Young's efforts to resolve the reparations imbroglio, and Ronald Swerczek's lucid description of Gibson's involvement in the diplomacy of arms limitation illustrate the persistent American engagement in European affairs within the constraints set by "corporatist" principles, domestic political pressures, and fears of strategic commitments. Frank Costigliola's interesting essay on John B. Stetson's ambassadorship to Poland underscores these themes but with an interesting twist. Costigliola shows how Stetson opposed the moderate revisionism tacitly endorsed in the State Department. As an alternative, Stetson supported the growth of American financial and economic power in Central and Eastern Europe. Such power in Stetson's grandiose view could be used to deter a potentially aggressive Germany, supplant Russian influence in Asia after the fall of the Bolsheviks, and enhance American interests vis-à-vis the industrial powers of Western Europe.

The essays on the 1930s elucidate the paralysis of American foreign policy and the growing frustration and impotence of American diplomats. This is especially evident in J. B. Donnelly's account of

Prentiss Gilbert. Even more insightful are Douglas Little's portrayal of Bowers's mission to Spain and Kenneth Moss's analysis of George S. Messersmith's reaction to Nazi Germany. These two diplomats foresaw the danger of Nazism and Fascism. Yet they were unable to convince most career diplomats of the need to take action to meet the emerging threat. Their failure was not simply the result of an "isolationist" mentality in Washington. It was also the consequence of the conservative orientation, limited vision, and anti-Bolshevik sentiment of most State Department officials. Loy Henderson typified this type of foreign service officer. And although Thomas R. Maddux makes a valiant effort to highlight Henderson's flexibility as a policy analyst, he succeeds mostly at demonstrating that Henderson's preoccupation with Soviet Russia prevented him from grasping the magnitude of the Nazi menace. Jane Vieth, in a partially sympathetic treatment of Kennedy, demonstrates that he preferred peace, even if it meant a Nazi-dominated Europe, to war because the latter might adumbrate chaos, the collapse of capitalism, and the spread of communism.

The major weakness of this volume, and it is not an inconsequential one, is the absence of a suitable introductory essay. The three-page preface by the editor and the foreword of two and a half pages by Alexander DeConde do not suffice to place the essays in any meaningful context. Indeed, there are major themes that run through this volume besides those of diplomatic frustration and failure that Jones and DeConde tend to emphasize. The essays highlight the economic assumptions, financial leverage, and corporatist nature of American foreign policy in the 1920s as well as the isolationism, appeasement, and anti-Bolshevism of the 1930s. The essays reveal the prominent role of business statesmen during the "New Era" as well as the growing professionalism and persistent conservatism of the foreign service during the New Deal. It is a pity that such themes and others could not have been elucidated in all their complexity and significance in an introductory chapter. With such an introduction and in a paperback edition, this volume could have widespread use in the college classroom.

MELVYN P. LEFFLER
Vanderbilt University

ALFRED E. CORNEBISE. *The Amaroc News: The Daily Newspaper of the American Forces in Germany, 1919-1923*. (New Horizons in Journalism.) Carbondale: Southern Illinois University Press. 1981. Pp. xxii, 248. \$24.95.

Between 1919 and 1923 a little-remembered newspaper known as *The Amaroc News* served the Ameri-

can forces of occupation stationed in the Rhineland. As a successor to *The Stars and Stripes*, its more celebrated forerunner, it deserves attention as a once lively medium of news, opinion, and entertainment. The *Amaroc* (the name is an acronym formed from the first two letters of each word in "American Army of Occupation") was, in many respects, a remarkable paper. Alfred E. Cornebise believes it was "essentially a military newspaper, reflecting the regular army ways which it often sought to inculcate within its readers" and that the officers who ran it produced a "noteworthy success" under difficult circumstances (p. 228). There is no reason to dispute that conclusion. Cornebise based his study primarily on an investigation of the back files of the *Amaroc* itself and, more than anything else, he presents a series of impressions from the pages of that paper.

These impressions are revealing in many ways. The editorial writing Cornebise describes covered a number of topics ranging from reflections on post-war life in the United States to criticism of the jazz craze and concern about alcohol abuse, venereal disease, and trafficking in cigarettes among the soldiers. Political themes also appeared. The editors, for instance, denounced "Bolsheviks" and "Radicals" and had considerable praise for President Wilson and the League of Nations. The author also provides numerous examples of the paper's staff features and leaves no doubt that columns such as "Say Buddie," "Vinegar and Violets," and "Ain't It So" added sprightliness to the *Amaroc*. The examples given of letters and poetry submitted by the soldiers themselves are similarly engaging. Just how much one can generalize upon such statements it is difficult to say. They are, however, valuable sources for social history and Cornebise should be commended for including them. Altogether, his effort to recreate the tone and character of the paper is successful.

In terms of presentation and interpretation of material, the book is less satisfactory. The brevity of several of its chapters suggests that a more effective division of material could have been employed. Moreover, the extensive use of long quoted passages, though justified to some degree, allows an otherwise engaging study to become tedious at times. More important, the concluding appraisal of the *Amaroc* found in the epilogue lacks thoroughness. Although the evaluation of the paper found there is reasonable, it fails to draw interpretations from the contents of the preceding chapters and makes little effort to place the *Amaroc* in a broad setting. Finally, with little exception, one is left wondering about the professional lives of the leading staff members after they departed from the paper. General Henry Tureman Allen, who commanded the American forces in Germany at this

time, thought the paper might be a type of "apprentice newspaper, training those who would continue to serve in journalism" (p. 62). This point would have been an interesting one to pursue.

The importance of this volume is twofold. First, it rescues from obscurity a modest journal that in its time became a locally important vehicle of communication and expression. Second, it provides a record of the attitudes and opinions of Americans soldiering in peacetime, as members of the American army of occupation in the critical years following World War I. Accordingly, the book makes a useful contribution to social history as well as to the history of journalism.

JAMES D. STARTT
Valparaíso University

JOHN ETTLING. *The Germ of Laziness: Rockefeller Philanthropy and Public Health in the New South*. Cambridge: Harvard University Press. 1981. Pp. x, 263. \$18.50.

This important book by a talented young scholar tells the story of hookworm disease in the South and the role of corporate philanthropy in combating it during the Progressive era. One of a species of parasitic worms known as helminths, the hookworm (or *Uncinaria americana*) came to the New World with the African slave trade. Hookworm infection occurs when the parasite enters the human body through the tender skin between the toes—hence the terms "ground itch" or "dew poison"—and eventually lodges in the bowel where it feasts on the host's blood. The victim perpetuates the cycle by depositing egg-laden feces on the surface of the ground. Hookworm disease, a debilitating anemia, is the result of repeated exposure to the parasite. The extensive prevalence of hookworm infection in the South was first brought to light in 1902 by Charles Wardell Stiles, a New York-born, German-trained zoologist employed in the Hygienic Laboratory of the United States Public Health and Marine Hospital Service. And it was through his efforts that the "germ of laziness" came to the attention of the Rockefellers.

By using skillfully drawn biographical portraits of John D. Rockefeller, Sr., and his chief advisor in matters of business and philanthropy, the Reverend Frederick T. Gates, both Baptists, John Etting explains the background of the Rockefeller Institute for Medical Research (1901) and the General Education Board (1903). These established interests in medicine and in Southern economic and social development through education, he argues, came together in the ensuing hookworm crusade. The author then focuses on the activities of the Rockefeller Sanitary Commission for the Eradication of

Hookworm Disease established by the oil titan for a five-year period in 1909 with a grant of \$1 million. Under the leadership of its administrative secretary Wickliffe Rose, the distinguished Tennessee educator, the commission kept the tarnished name of Rockefeller in the background while it mounted an extensive campaign against hookworm from Virginia to Louisiana through the state boards of health. In funding the salaries of state medical directors, local sanitary inspectors, and field and laboratory microscopists, the commission sought to ascertain the extent of infection, treat and cure the victims, and prevent reinfection by promoting adoption of the privy. Although "eradication" was by no means achieved, Etting concludes on balance that the commission's accomplishments were commensurate with its goals. Most important, the commission greatly strengthened public health agencies in the South, and its strategies there shaped the global objectives of the Rockefeller Foundation (1913).

In the presentation of his evidence, the author challenges effectively the narrow Marxist interpretation offered recently by E. Richard Brown in *Rockefeller Medicine Men: Medicine and Capitalism in America* (see *AHR*, 75 [1980]: 727–28). Rather than an expression of dominant class interests, Etting finds the motivations of Rockefeller, Stiles, Gates, Rose, and the Southern actors in the hookworm drama deeply rooted in the soil of nineteenth-century evangelical Protestantism. The study has been awarded (a slight irony here) the prestigious Allan Nevins Prize. It is a shame that the book is marred by so many typographical errors.

JOHN H. ELLIS
Lehigh University

W. DIRK RAAT. *Revolteros: Mexico's Rebels in the United States, 1903–1923*. College Station: Texas A&M University Press. 1981. Pp. xviii, 344. \$22.50.

W. Dirk Raat's fascinating narrative describes how, in the early twentieth century, the United States participated in the international exchange of information regarding groups that the ruling elite in the United States considered "dangerous" radicals. Perhaps the major conceptual contribution of Raat's work lies in the clearly revealed relationship of domestic and foreign policy. Exposing the uses of state authority to control the international movement of "ideas" well before the Bolshevik revolution represents another valuable contribution of Raat's study. The assembled evidence also convincingly supports Raat's contention that the United States unmistakably intended to influence Mexican internal politics, thus undermining any argument for "accidental" interrelationship between foreign and domestic affairs.

Raat describes a variety of activities that state and federal officials in the United States entered into in order to destroy or weaken allegedly dangerous radical groups, such as the Magonistas. These "radical" groups, Raat argues, were identified through a veiled decision-making process within the upper and middle ranks of the national governing elite. Finally, Raat also discovered that when a group arose that supposedly represented no challenge to the internal sense of order and stability of American society, its international role was "accepted" and, consequently, the United States found little basis to hinder its activity. Thus United States government agencies willingly violated United States law in order better to aid Porfirio Díaz against the Magonistas; the reverse was not seriously considered. When Enrique Creel, whom Díaz had entrusted with directing the international spy ring in cooperation with the United States government, fled to the United States after Díaz's fall and began to plot against the Madero regime, the United States government did not treat him as a "revoltoso."

Several studies can complement or extend Raat's argument, such as Ronald Radosh's *American Labor and United States Foreign Policy*, John Hart's *Anarchism and the Mexican Working Class*, and Robert Hilderbrand's recent monograph, *Power and the People: Executive Management of Public Opinion in Foreign Affairs, 1897–1912*, which describes the executive branch's striving to control and manipulate information regarding foreign relations.

Raat has given us a well-written, valuable monograph. Based on extensive research in Mexican and United States archives and published documents as well as private papers, memoirs, newspapers, and journals, this monograph takes the reader into areas of history not often traveled. This fine study should long prove valuable to historians of Mexico and the United States regardless of whether they specialize in domestic or foreign affairs.

THOMAS SCHOONOVER
University of Bielefeld

WILLIAM LEACH. *True Love and Perfect Union: The Feminist Reform of Sex and Society*. New York: Basic Books. 1980. Pp. xii, 449. \$18.00.

ELAINE TYLER MAY. *Great Expectations: Marriage and Divorce in Post-Victorian America*. Chicago: University of Chicago Press. 1980. Pp. viii, 200. \$15.00.

William Leach's intention in *True Love and Perfect Union* is to challenge prevailing views on the attitudes of nineteenth-century feminists. He rejects the notion that leaders in the fight for female advancement and suffrage shared an ideology of woman's special moral character or looked to ex-

pand a particularly feminine sphere of influence. Instead, Leach insists that feminists "championed equal educational and economic opportunity for both sexes in the public realm" (p. 3). They were seeking to blur sexual differences and foster an "egalitarian ideology" (p. 9) that would bind the sexes together.

Although the argument is novel, it is neither satisfactorily documented nor cogently argued, and in too many ways Leach's own evidence undercuts his assertions. First, he swells the category of feminist beyond recognition; almost anyone who makes a statement on the value of sexual equality qualifies for the label. Franklin Sanborn, best known as a prison reformer, is to Leach an "important feminist" (p. 134). Why? Because he once noted that social science was "woman's work" (p. 316) and helped to found the American Social Science Association, which "brought men and women altogether in a common cause" (p. 297). Indeed, anyone associated with the association earned the designation. The end result is such an insensitivity to degrees of commitment as to make the feminist category meaningless.

Leach is also forced to minimize the role of traditional feminist leaders, particularly those who did look to expand women's special sphere of influence. Francis Willard barely enters the monograph. The WCTU is dismissed as a one-issue organization, *not much more important than a dress reform society*.

One of Leach's major arguments is the claim that true feminists supported only institutions of learning where women could join men in obtaining a fully equal education. Already by the 1860s and 1870s university coeducation, Leach maintains, was a "prerequisite for successful marriage" (p. 69). Seemingly, the position went beyond theory to practice, for Leach declares that in these decades coeducation came to "full and enduring fruition," standing as the "mark of the reconstruction of the social life of the North" (p. 72). But just what this fruition represents is unclear. Leach quickly concedes that "most of the coeducational institutions of higher learning ranked in quality far below the male schools (and later, below such female schools as Vassar and Smith)" (p. 72). And this circumstance continued into the 1880s, for only 11 of the nation's 208 coeducational institutions were accredited. Leach devotes special attention to Wesleyan's president, John Vleck, who promoted coeducation as a "feminist experiment" (p. 75). Under Vleck's leadership, "Wesleyan became the first eastern college to institute egalitarian coeducation" (p. 74). But in fact, between 1873 and 1980 Wesleyan only admitted two to six women students annually. By 1909 it had eliminated women students entirely and would only readmit them in 1972.

Leach has surprisingly little sense of the character of coeducation at other colleges. Michigan and Wisconsin treated their female students as second-class citizens; Wisconsin admitted women to the normal school but only very slowly integrated them into the rest of the university. As for coeducation being a prerequisite for successful marriage, all Leach can do is note that about half of the women who attended Wesleyan married Wesleyan men and come up with a few quotations that promote coeducation as a way of uniting the sexes. Eventually, even Leach comes round to argue that "coeducation improved possibilities for better marriages" (p. 80), but even that limited formulation is not persuasively documented.

In the end, Leach's book makes traditional arguments appear even more valid. Feminists were seeking to enlarge and advance women's spheres. It may be novel to deny this interpretation, but it is certainly not sound.

Elaine Tyler May's *Great Expectations* is an effort to explain the dramatic rise in divorce rates in the late nineteenth and early twentieth centuries by analyzing what "post-Victorian Americans seemed to want out of marriage and why they were increasingly unable to achieve it" (p. 7). Her primary source is the records of 1,250 divorce proceedings, 500 in the 1880s, another 500 in 1925 from Los Angeles, and 250 in 1920 from New Jersey. She examines quantitative as well as qualitative data, reviewing both the socioeconomic status of those who obtained divorces and the couples' explanations for their action as described to the court. In Los Angeles, for example, May discovered that lower-white-collar groups were overrepresented among the divorced, a finding she attributes to the fact that small and independent shopkeepers felt threatened by the competition of new department stores. The link is not very persuasive, moving from the personal to the global too quickly, and it does seem a bit late to be applying "status panic" to divorce.

May's qualitative analysis provides a fascinating portrait of the individual couples, but again she has difficulty in moving from the anecdotal to the general. She argues, for example, that in the 1920s the inability of husbands and wives to reconcile new values—such as material consumption and sexual freedom—with their lingering desire for a traditional home life promoted divorce. But new values are often in conflict with old ones—can one think of an era where this was not true?—and yet a rise in divorce was not invariably the result. It may also be that May's couples were telling the court what the sociologists and popular writers of the period had, in effect, told them to say. It had become legitimate, maybe even necessary, to cite sexual incompatibility and rising personal expectations as the reasons for ending a marriage. Nevertheless, with these reserva-

tions, May deserves credit for making us aware of an important and neglected source of data for family history and women's history. Recapturing parts of the world of anonymous Americans is not as hopeless a task as it has appeared.

SHEILA M. ROTHMAN
Columbia University

PHILIP S. FONER. *Women and the American Labor Movement: From World War I to the Present*. New York: Free Press or Collier Macmillan, London. 1980. Pp. vi, 682. \$22.50.

The second volume of Philip S. Foner's massive study of women and the labor movement covers the period from the First World War until the present. Foner has a genuine narrative gift. Despite its length, his book held this reader's interest from beginning to end. Some of the chapters—for example, those on the repression of the left during and after the First World War, the great CIO strikes of the 1930s, and the organization of farm, textile, and hospital workers since the Second World War—are genuinely stirring. Foner's documentation is also impressive. His bibliography and notes, covering almost a hundred pages, include both published and unpublished sources and take account of the most recent work on women workers and trade unionists. Throughout, footnotes provide biographical data on persons mentioned in the text and other essential background information. Foner's study will undoubtedly be a basic source in the field for years to come.

Foner makes no effort to hide his sympathies. They lie with the most exploited groups in the American labor force—unskilled workers, members of minority groups, and women—and the political radicals who tried to organize them. Indeed, if there are any heroes in this book, they are the Communists. Foner portrays them as the most consistent opponents of racism and sexism in the labor movement and the most courageous, tenacious organizers of the oppressed. Their dedication and militance were essential ingredients in the CIO successes of the 1930s, while the purges of the 1950s marked the end of the CIO's role in the vanguard of the struggle for social and economic justice. Foner condemns the AFL and its early leader Samuel Gompers for racism and sexism and sees AFL policies throughout the period as an obstacle to unionization and the interests of the majority of American workers.

Within this interpretative framework, Foner focuses on the woman worker. His general picture of her position in the economy and her relationship to unions does not differ markedly from that which appears in recent general works on women's history

such as William Chafe's *The American Woman, 1920–1970*, or more specialized studies such as Maurine Greenwald's *Women, War and Work: The Impact of World War I on Women Workers in the United States*. Indeed, Foner explicitly endorses Greenwald's view that World War I altered patterns of female employment without causing any fundamental change (p. 23). What Foner's richly documented account accomplishes is threefold. First, he brings together an enormous amount of information on women active in trade unions and on strikes (both successful and unsuccessful) in industries where women were the majority of the labor force. In this connection, he is to be particularly commended for his attention to black and Hispanic women. Second, he argues convincingly that the barrier to organizing women lies not in the indifference of female workers to unions or their unwillingness to endure the hardships of strikes, but to the brutal opposition of corporations and local governments and to sexist attitudes in the unions themselves. Finally, he demonstrates that today unions need women as much as women need unions. In an era when the percentage of organized workers is falling, unions ignore the growing number of female workers at their peril, while women, who still work for lower wages in worse conditions than men, need the protection that only trade unions can give them.

BARBARA J. HARRIS
Pace University

KAREN ANDERSON. *Wartime Women: Sex Roles, Family Relations, and the Status of Women During World War II*. (Contributions in Women's Studies, number 20.) Westport, Conn.: Greenwood Press. 1981. Pp. 198. \$22.95.

WINIFRED D. WANDERSEE. *Women's Work and Family Values, 1920–1940*. Cambridge: Harvard University Press. 1981. Pp. 165. \$18.50.

In the last dozen years the book-length histories of American women have principally focused on the late eighteenth and nineteenth centuries. These studies have explored family life and female culture; women's employment in the home, factory, store, and office; and women's contributions to the antislavery, temperance, birth control, moral reform, labor, and women's rights movements. Monographs on twentieth-century women's history in the United States are finally beginning to appear. These two well-researched books respectfully redirect our attention to the importance of economic development and major wars for understanding women's wage work and home life from the 1920s through the 1940s.

Winifred D. Wandersee's *Women's Work and Family Values* thoughtfully probes the meaning of the

gradual but steady increase in participation of white married women in the labor force between 1920 and 1940. Based on a closely reasoned assessment of economic, demographic, and sociological literature, Wandersee's book explores the relationships between married women's employment patterns and well-known structural and value changes in interwar America. According to Wandersee, the increase in married women's paid employment should be attributed to the redefinition of the American standard of living from the basics of decent housing, food, and clothing to anything enhancing one's life, including commercial entertainment, domestic appliances, and automobile ownership. Wandersee suggests that the new definition of economic need resulted from three features of American life originating in the 1920s: the mass production of new consumer goods, large-scale advertising, and installment buying. In addition, the interwar decline in both the birth rate and child labor combined with a marked increase in white-collar jobs to encourage women to seek wage work.

Wandersee logically follows married women's employment trends into the 1930s when the Great Depression caused a slowdown in the movement of married women into office and professional work as private and public employers introduced measures to discourage or prohibit the hiring or retention of married women. Nonetheless, wage work among married women continued to grow during the depression, as wives attempted to maintain their families' standard of living. Despite the adverse effects of the depression on many households, Wandersee finds that consumption patterns established before the collapse of the economy continued unabated during the 1930s among families not dependent on relief. Especially illuminating is Wandersee's use of sociological studies of family life during and after the depression to discuss the complex effects of changes in American economic life on women's roles in the family.

Finally, in a flight of speculation, Wandersee suggests that married women's emphasis on their families' welfare may explain why feminists of the 1920s and 1930s, who were concerned with women's personal and economic independence, did not win mass support for their cause. In her exploration of women's attitudes toward their family and work roles, Wandersee pays little attention to the nature of most women's wage work. It may also be the case that the very character of women's employment in routine, dead end, and low-paying jobs discouraged the mass of female white-collar workers from identifying with the concerns of women employed in more prestigious and better-paying professional and business jobs.

There is particularly one respect in which Wandersee's work begs clarification. By limiting her

discussion to white women, she leaves unanswered implicit questions about the effect of twentieth-century economic changes on black women and their families. Since participation of black married women in the labor force always exceeded that of white married women on a proportional basis, one wonders what effects the rise of a consumer economy, prohibitions against child labor, and a falling birth rate had on black family life and employment patterns.

Karen Anderson's *Wartime Women* continues the story of women's work and home life through the period of the Second World War, focusing on three major areas of defense production—Baltimore, Seattle, and Detroit. Building on works by Eleanor Straub, William Chafe, Susan Hartmann, Lisa Anderson and Sheila Tobias, Alan Clive, and D'Ann Campbell, *Wartime Women* attempts to explore in greater depth women's employment experiences, family life during wartime, and the problems of child care and sexuality.

Anderson takes strong exception to the view that the Second World War offered women unprecedented economic opportunity and social advancement. Instead, she justly concludes that the war altered the occupational distribution of women, increased the number of married women in the labor force, and only *modestly* improved the overall status of women in American society. In Seattle, Detroit, and Baltimore, employers and unions only reluctantly opened their doors to women, especially in previously male-dominated workplaces and occupations. Even worse, black women continued to be objects of discrimination based on race as well as gender.

By focusing on war-boom areas Anderson offers an authentic portrait of communities suddenly rent by multiple crises—massive shortages of labor, housing, schools, consumer durables, and foodstuffs. Generalizations based on Anderson's study of women's daily work and home life must be highly qualified, however, since the number of centers of defense production such as the three studied here was relatively low.

Anderson's composite picture of changes in war-boom areas is punctuated by a tantalizing catalogue of differences among the three locales. For example, she notes that Detroit automakers did not hire as many women as Seattle aircraft manufacturers, even though the automakers confronted equally severe labor shortages. Detroit employers also stand out in contrast to Baltimore and Seattle employers as being particularly hostile to the hiring of black women throughout the war. In a similar vein, Anderson observes that Seattle shipbuilders, boiler-makers, and taxicab drivers resisted the admittance of women into their unions, whereas in other places women were more readily received into trade

unions in those occupations. Unfortunately, Anderson does not adequately explain the reasons for these local variations—whether due to workplace or labor movement traditions, managerial manipulations, or technological changes threatening to male workers.

Anderson's descriptions of family life and child care in wartime are particularly insightful. She offers a sensitive and well-documented portrait of the many social problems facing families during wartime, such as housing shortages, lengthy work days, juvenile delinquency, teenage employment, sexual conduct, and day care needs. Local and federal sources are artfully combined to render a rich and varied picture of women's reactions to wartime changes. The problem remains, however, to explain local differences in day care programs, the regulation of sexual behavior, and other social phenomena; description brings one only so far.

Anderson's conclusion is compatible with Wandersee's thesis. While the Second World War did not substantially redefine women's status in American society, it contributed to the legitimization of married women's right to work outside the home in order to bolster their families' financial security and material accumulation. Married women's assessment of their dual role in post-World War II America still remains to be explored.

MAURINE WEINER GREENWALD
University of Pittsburgh

GIAN GIACOMO MIGONE. *Gli Stati Uniti e il fascismo: Alle origini dell'egemonia americana in Italia*. (Biblioteca di storia contemporanea, number 456.) Milan: Feltrinelli Editore. 1980. Pp. 404. L. 13,000.

Gian Giacomo Migone has written a subtle and provocative study of the cooperation of American bankers (primarily the House of Morgan), the United States government, and the Fascist regime in the post-World War I political and economic stabilization of Italy. Using an impressive range of American, British, and Italian manuscripts, together with interviews, Migone argues that American economic aid was a critical element in the consolidation of the Fascist regime in Italy. Mussolini oriented both his foreign policy and internal economic program to attract the backing of American bankers and United States policy makers. Both groups viewed political stability in Europe as an essential precondition to a massive expansion of American investment and production abroad and willingly supported any regime that could provide order.

The central elements in the Italo-American rapprochement were the settlement of the Italian war debt on terms highly favorable to Italy in 1925 and the American-financed stabilization of the lira

in 1927 that Migone argues solidified Mussolini's rule. In return, Italy supported American efforts to organize the world economy under United States hegemony. Fascist cooperation reached its apogee during the Hoover administration before abruptly ending in late 1931 when the effects of the depression undercut American economic power. Mussolini jettisoned the American alliance and embarked on a program of economic nationalism and rearmament.

The former allies drifted further apart during the first Roosevelt administration. The Americans continued to promote disarmament and free trade as the basis of world order. Italy's attack on Ethiopia in 1935 strained relations to the breaking point. Roosevelt's efforts to block Fascist aggression were effectively checked by organized political pressure orchestrated by American bankers. Nevertheless, U.S. bankers distrusted Mussolini's new orientation and, when unable to restrain him, they reluctantly abandoned the Italian dictator.

Migone's study fills an important gap in the scholarship on the political and economic relationship between the United States and Italian Fascism, nicely complementing John Diggins's *Mussolini and Fascism* (1972) on the intellectual and cultural interaction. It is a model of organization, weaving the strands of diplomacy, high finance, and ethnic politics into a coherent pattern and is written with a style often lacking in Italian (and American) historical works.

Nevertheless, the book's most original thesis is untested and unproven. Migone, like other Marxist historians, is intent on refuting Renzo De Felice's argument that the Fascist regime enjoyed at least the passive support of all classes from the late 1920s through the Ethiopian war. He offers American aid as an alternative explanation for the regime's longevity. American aid certainly reinforced the hold of Fascism, but the regime rested on the triad of force, mass organization, and compromise with the traditional ruling elite. In fact, Mussolini reached the height of his popular support after he broke with the United States to follow an independent course in foreign and domestic affairs and before he entered into his fatal alliance with Hitler.

In spite of these objections, *Gli Stati Uniti e fascismo* is a book of singular merit and its translation into English would greatly benefit a wider body of scholars interested in American economic diplomacy in the 1920s and 1930s.

JAMES EDWARD MILLER
Department of State

ZANE L. MILLER. *Suburb: Neighborhood and Community in Forest Park, Ohio, 1935–1976*. (Twentieth-Century America Series.) Knoxville: University of Tennessee Press. 1981. Pp. xxxi, 263.

Although the United States has become a preeminently suburban nation in the decades since World War II, the literature on the history of suburbanization and suburban life is still relatively sparse. In his new study, Zane L. Miller has tried to add some structure to this incomplete portrait of the suburban past by depicting the origins and development of Forest Park, a planned community on the fringe of Cincinnati. Miller claims to be writing what he calls a "symptomatic" history in which he treats events in this single suburb as symptomatic of American conceptions of community during the past four decades. Thus the history of this one community supposedly provides indications of changing American attitudes, allowing the historian to diagnose not only Forest Park but also suburban America at large.

Miller identifies three different periods of suburban perception between 1935 and 1976, each period distinguished by what Miller calls a "taxonomy of social reality." First, there was the era of the metropolitan mode of thought that lasted until about 1950. During this era suburbanites viewed the metropolis as the basic unit of society and still retained a community of interest with the central city. Second, there was the "community of limited liability" characteristic of the period between 1950 and the mid-1960s. At this time the distinction between central city and suburb grew sharper, but the increasingly autonomous suburbs served a mobile population with fewer territorial ties. Third, there followed the era of the "community of advocacy" from the mid-1960s through 1976 characterized by neighborhood organization and what Miller calls a "turning-inward tendency."

Using these concepts Miller describes the history of Forest Park from the developer's purchase of the land, through the first years of settlement and the founding of the original civic associations. He proceeds to examine the community's incorporation as a village, its political strife, and the transfer of civic responsibilities from the private associations to the public agencies of government. And then he recounts the reaction of Forest Park's government and community organizations to the increase in black population during the early 1970s and to the perceived threat of urban blight. Throughout, Miller provides detailed information on the public and private governments of the community, and in fact this is not so much a history of Forest Park as a history of its government. Miller's book says little about the actions, attitudes, or life style of the majority of Forest Park residents who cared so little about the community that they did not even bother to vote in municipal elections. He presents a history of the community's activists, those who chose to govern.

Thus Miller offers a detailed account of commu-

nity rule, identifying the "symptoms" of changing social reality. One might question whether the events in Forest Park are symptomatic of anything other than the local conditions of suburban Ohio. And one might dispute the validity of Miller's "taxonomies." But Miller has provided future historians a framework to consider and challenge, and no doubt his work will stimulate further investigation in the vital field of suburban history.

JON C. TEAFORD
Purdue University

DAVID M. TUCKER. *Memphis since Crump: Bossism, Blacks, and Civic Reformers, 1948-1968*. Knoxville: University of Tennessee Press. 1980. Pp. xvi, 183. \$10.50.

David M. Tucker's brief analysis of Memphis in the post-World War II era offers a provocative look at contemporary urban reform movements. He begins with a short sketch of twentieth-century Memphis politics highlighted by an acid portrait of Edward H. Crump, the dominant politician in the city from his election as mayor in 1909 to his death in 1954. Tucker's emphasis on Crump's ruthlessness, his underworld connections, and his intolerance of dissent contrasts sharply with William Miller's more benign interpretation of the "Boss" in his biography, *Mr. Crump of Memphis* (1964).

Crump's dictatorial methods, Tucker contends, provoked opposition among liberal elements of the city's business and professional classes who were drawn into politics in the mid-1940s by their commitment to world government. Typified by such men as newspaper publisher Edward Meeman, attorney Lucius Burch, and businessman Edmund Orgill, this group was horrified by the recent totalitarian movements in Europe and determined to prevent the devastation of another global conflict. Deeply influenced by Clarence Streit's book, *Union Now*, these men and women were convinced democracy could be preserved only within the framework of a federal union of nations. This rather visionary goal brought them into conflict with Crump when they endorsed Estes Kefauver in the 1948 Tennessee Senate race. Although Orgill and his associates supported Kefauver primarily for foreign policy reasons, Crump was outraged to find prominent citizens of Memphis daring to support his bitter enemy. The political and economic pressure Crump unleashed against them clearly dramatized the kind of power Crump could wield and awakened the fledgling reformers to the threat to democracy that seemed to exist in their own city.

The struggle between the reformers and the Crump machine was conducted against the backdrop of rising discontent in the black community.

After Crump's death, Edmund Orgill was elected mayor in 1955 partly because his reform campaign was able to attract substantial numbers of black voters, but by the early 1960s, Memphis politics was deeply polarized along racial lines. Convinced that moderate policies of Orgill and his associates were ineffectual, younger black leaders demanded the white elite do more to combat poverty and injustice in Memphis. Simultaneously, white resistance to black pressure fostered the growth of a conservative Republican party adamantly opposed to concessions on racial matters. The angry division climaxed in the bitter 1968 garbage workers strike and the assassination of civil rights leader Martin Luther King, Jr. The politics of race, Tucker concludes, overwhelmed the reformers and ultimately frustrated their plans for the city.

Tucker's conclusions reflect the general disenchantment many scholars are currently expressing with mid-century liberalism. Unable to manage the city's growing tensions, the Memphis reformers failed as political leaders and were repudiated by both blacks and whites. Tucker's interpretation of this process is persuasive, but the book is marred somewhat by the author's rather partisan tone, an anti-Crump attitude that sometimes leads him to exaggerate both Crump's power and his venality. His intriguing thesis that events abroad sparked reform at home is a novel explanation that needs additional testing in other studies. Tucker is not altogether persuasive on such points, but he has written a valuable overview of the rise and fall of a postwar reform effort.

DAVID D. LEE
Western Kentucky University

WILLARD B. ROBINSON. *Gone from Texas: Our Lost Architectural Heritage*. (Centennial Series of the Association of Former Students, Texas A&M University, number 9.) College Station: Texas A&M University Press. 1981. Pp. xix, 296. \$29.95.

It is a testament to Willard B. Robinson's definition and treatment of the architectural heritage of Texas that one wishes he had concluded his book with a fuller chapter on the twentieth century—particularly the period since World War II. Certainly one of the larger implicit messages of his work is that the growth and professionalization of the historic preservation movement within the state of Texas is part of a national cultural development rooted in the dramatic changes that transformed so much of the American landscape in the postwar period. It is precisely because most of the book is so successful in relating architecture to history that one ends the work wanting to know more about the historic context of recent attitudes about architecture in

such a critically important "sun-belt" state where so many of the buildings Robinson describes and illustrates were lost to "progress."

Gone From Texas: Our Lost Architectural Heritage is much more than a guide or index to individual structures and buildings lost to development, fire, or natural catastrophe. While the book is meant to appeal to the same type of audience that read Nathan Silver's influential book, *Lost New York* (1967), Robinson, responding to the growth and change of the historic preservation movement, has broadened his approach and raised the level of historic understanding. Like Jane Holtz Kay in *Lost Boston* (1980), Robinson, without diminishing the importance of the artifact, treats buildings of Texas as part of national architectural development and architecture as interrelated with history.

In seven well-written, documented, and illustrated chapters, Robinson sets out the periods of Texas architecture from the pre-European settlements of native Americans through the twentieth century. By defining architecture broadly, the author is able to include fascinating discussions of city planning, technological innovations such as cast iron and the elevator, and industrial structures such as bridges. Simultaneously, he provides a more formal analysis of Texas architecture according to accepted stylistic interpretation: Greek Revival, Queen Anne, Romanesque, and Beaux Arts. This is all done within a chronological and thematic development of material in which he consistently is aware of the larger historic context within which architecture fits. Robinson treats succinctly such historic contexts as the period of Spanish settlement, the establishment of utopian socialist communities in western Texas—which so influenced the journalist of the antebellum South, Frederick Law Olmsted (pp. 31–33)—the growth of ethnic communities, the effect of the Civil War and railroad development on Texas building, the spread of the Chautauqua movement, and the era of the large resort hotels.

It is precisely because the author is so successful in relating architecture to its natural and logical historical contexts that one misses more discussion of the last thirty years of Texas history. For surely Robinson is stating a truth shared by an ever-widening number of architectural and other historians when he writes that "the history of architecture is much more than an account of the works of anonymous builders and talented designers or a discussion of styles. Rather, it should be a survey of conditions, both objective and subjective, influencing those works: environment, society, art, technology, and individuals" (p. xix).

In this regard, the entire perspective of the historic preservation movement—and of this book—including a search for a usable past of planning and design "more energy efficient than today's

structures" (p. xviii), is a function of the post-World War II period. The book would have profited from more attention to this period within which the cities of Texas—particularly Houston—and their architecture became so prominent nationally. Nevertheless, because of its many merits, including an excellent bibliography and glossary of architectural terms, *Gone From Texas* is essential reading for all citizens of the Lone Star State concerned with their built environment, as it is for all seeking local examples of national developments. Given the lunatic prices of books today, it is a bargain and a handsome book to boot!

ALBERT FEIN
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ABRAHAM HOFFMAN. *Vision or Villainy: Origins of the Owens Valley-Los Angeles Water Controversy*. (Environmental History Series, number 3). College Station: Texas A&M University Press. 1981. Pp. xix, 308. \$18.50.

This book should appeal to three groups: Californians interested in the complex story of Los Angeles's water supply, readers who enjoy a good work of history regardless of where they live, and historians who will relish the noble contribution of Abraham Hoffman. He exposes the superficial, grossly distorted, and often false story of the Owens Valley-Los Angeles controversy as presented by journalists, polemicists, and, regretfully, by a few rather amateurish historians. Hoffman shows how thorough research into primary sources, dispassionate analyses of controversial events, and careful weighing of evidence can expose myths about conspiracies and corruption.

Hoffman notes the city's boosterism, its resultant growth, and its concomitant need for more water. Into the story come the principal characters: Joel Barlow Lippincott, who was probably guilty of conflict of interest but felt he was working "for the greatest good of the greatest number"; William Mulholland, hard-driving head of the Los Angeles Department of Water and Power; and Frederick Eaton, one-time mayor of Los Angeles, surreptitious purchaser of Owens Valley lands, and stubborn landowner of a prime reservoir site. Each of these key figures is the subject of an excellent thumbnail biography.

The complex story, which involves the taking of the water supply of the Owens Valley, 240 miles to the north, by aqueduct to thirsty Los Angeles, is a fascinating study involving the compromising of ethics because the growing city *had* to have water. The stakes were high; land speculators, bankers,

and politicians were involved; and the few residents of the towns, farms, and ranches of the Owens Valley fought a losing battle against overwhelming odds. Eventually Los Angeles owned about ninety-eight percent of the private land in the valley. In the process of acquiring it—and the accompanying water rights—the Bureau of Reclamation, the Los Angeles Department of Water and Power, and virtually all the main characters in the story, in addition to many other persons, had their names besmirched. Often they were victims of accusations that Hoffman proves false.

For the historian the chapter entitled "Andrew Nordskog and the Persistence of Conspiracy History" is the most interesting. Hoffman traces the growth of the conspiracy and the accusations. He shows how literally dozens of writers have continued the distortions. Usually they have trusted in the polemical tract written by Nordskog, a publicist of the conspiracy theory, who in researching the story to substantiate his predetermined conclusions violated every known rule of historical research. Hoffman with thoroughness and impartiality exposes Nordskog's errors. He makes historians proud of their profession, and proud of him as a valued member.

This book presents the complex story of the Owens Valley-Los Angeles controversy as impartially as it could be accomplished given the nuances, side issues, and characters involved. It is history at its best: well researched, well organized, impartial, and well written. A map and twenty-eight pictures add interest. Hoffman has done justice to a great story.

RICHARD A. BARTLETT
Florida State University

PAOLO E. COLETTA. *The United States Navy and Defense Unification, 1947-1953*. Newark: University of Delaware Press. 1981. Pp. 367. \$32.50.

For a number of reasons it was evident in 1945 that the American government was going to have to change the process by which it conducted its national security affairs. These reasons included: the new world role of the United States resulting in the need for forces in being as contrasted to the previous mobilization concept; and the need before long for someone to make hardware decisions among the competing service views on using new technological possibilities.

Proposals for a new organization for national security affairs were debated between 1945 and 1947, largely but not exclusively, in the congressional forum. The outcome was the National Security Act of 1947 that created four major agencies: the

National Security Council; the Central Intelligence Agency; the National Military Establishment (NME) to be headed by a secretary of defense; and a separate air force within the NME.

Most of the debate centered on what kind of National Military Establishment should be established. The army wanted a highly centralized defense establishment and set forth its view in the Collins plan. The navy on the other hand led by its secretary, James Forrestal, wanted a loosely federated arrangement, and set forth its views in the Eberstadt report. The navy view won.

Subsequent events, especially the experience of the first secretary of defense, James Forrestal, made clear that the defense secretary needed considerably more authority than the 1947 act had provided. Legislation in 1949, which among other things changed the NME to the Department of Defense, Eisenhower's Reorganization Plan Number 6 of 1953, and the Defense Reorganization Act of 1958 provided the legal means for future secretaries to truly control the Defense Department.

Paolo E. Coletta's book concerns the struggle toward unification of the defense establishment from its origins in 1947 through 1953. He tells the story from the navy perspective, the service which opposed the increasing control of the defense secretary. Most of the book is concerned with the period of the incumbency of Secretary of the Navy John L. Sullivan, from 1947 to April 1949 and Francis B. Matthews, his successor, whose tenure ended in July 1951. There is some brief coverage of the tenure of the two later secretaries into 1953.

The book is rich in factual material and makes the unification struggle come alive. This struggle undoubtedly reached its peak during the time Louis Johnson was secretary of defense. Economy-minded Johnson's scrapping of the supercarrier *United States* resulted in the resignation of Secretary of the Navy Sullivan. The role of the carrier and the role of the air force's B-36 were the substantive aspects of the ensuing public debate. The real issues were the allocation of resources among the services, and how much authority the secretary of defense should have. The subsequent "Revolt of the Admirals" is well told by Coletta.

The perspective of the book is interesting, coming from the point of view of a particular service. The research is impressive, and the author's insight into the problem is evident. Though the writing style leaves something to be desired, it should not deter those interested in the period or the subject.

My main criticism of the book is that the author carried it too far. The early Eisenhower period is covered in nine pages. Such a superficial coverage does more harm than good and probably could have been left out to the advantage of the book. All

in all, Coletta has done a first-rate job on an important topic.

DOUGLAS KINNARD
University of Vermont

MORRIS J. MACGREGOR, JR. *Integration of the Armed Forces, 1940-1965.* (Defense Studies Series.) Washington: Government Printing Office. 1981. Pp. xx, 647. \$17.00.

The close relationship between the military and American society is clearly illustrated in Morris J. MacGregor's excellent account of the process of racial integration. The study describes the pressures that shaped the military's decisions on black manpower use. It is a well-balanced presentation of the process that by 1965 had moved the armed services to a position of leadership in the area of equal opportunity for blacks.

MacGregor's book considers two eras of race relations: the culmination of the drive for an end to segregation (1940-55) and the beginning of the effort toward equal opportunity (1960-65). The first chapters describe the segregation policies of the World War II period and the few deviations from them. The services hoped to continue the racial status quo in the postwar era, but a variety of pressures forced changes. MacGregor makes it clear why the air force and navy moved fairly rapidly toward integration while the army and marine corps did not. In the 1960s the civil rights movement, encouraged by the earlier military desegregation, pressured the armed forces to move toward equal opportunity for all. Again MacGregor describes the forces for action and inaction inside and outside the Defense Department. The book concludes with an analysis of what had been accomplished in the drive for equality and the major problems that by 1965 were unresolved.

This book is an excellent account of the struggle to place black enlisted men and officers into the mainstream of the American military establishment. It builds upon the work of other writers, such as Ulysses Lee and Richard Dalfiume. It is, however, much more comprehensive than any previous detailed work both in terms of the time period covered and the services involved. Based on a wide variety of interviews and primary sources, it is a broad examination of all four services over a twenty-five-year time span. The author effectively weaves in those national political developments and events in the black community that had an impact on the military. The only drawback to the work is the omission of any discussion of the activities of black enlisted men and officers, which leaves the reader wondering what was happening in the field while the policy

makers talked and argued. The many photographs partially help to fill this gap. Though this is a government publication, it is not an apology for the mistakes of the military leaders. It clearly indicates where they failed in the field of race relations and why. For anyone interested in the issues of integration and equality in American society since 1940, this book will be a valuable addition.

MARVIN FLETCHER
Ohio University

JODY CARLSON. *George C. Wallace and the Politics of Powerlessness: The Wallace Campaigns for the Presidency, 1964–1976*. New Brunswick, N.J.: Transaction Books. 1981. Pp. xv, 331. \$22.95.

Why did people vote for Wallace? Was his movement in any proper sense "fascist"? How was it organized? Did it have an ideology? Could some other demagogue rally his supporters again? Hunter College sociologist Jody Carlson answers these questions through a re-analysis of survey data gathered by others, and of Wallace speeches and campaign documents, interviews with Wallace staffers, and a perusal of secondary literature and newspaper stories—nearly all of the latter from the *New York Times*. Marred by a wooden writing style, a mechanistic organizing scheme, and extremely unsophisticated statistical data analyses, Carlson's book is objective and often interesting and will be a considerable resource for future historians of the "New Right."

Of Carlson's sixteen chapters, the nine that sketch Wallace's early career, narrate his 1964–76 national campaigns, and recount the themes of his speeches in each race contain few surprises for anyone who followed the national media at the time. The heart of the book is an explication and a series of tests of four social-psychological hypotheses about Wallace supporters, which she labels the "authoritarian personality," "marginal voter," "status politics," and "power" theories. Although there were few survey items drawn from Adorno's "F scale," Carlson argues that the much greater tendency of Wallace supporters than other voters to favor a military solution to the Vietnam war and harsh treatment for civil rights and antiwar protesters indicates their basic authoritarianism. Drawn from all social classes, as likely as other people to participate in, and to pay attention to, politics (at least when provided with candidates who share their views), and not notably anxious about their status or extraordinarily socially mobile either up or down, the Wallace supporters do not fulfill the expectations of the "marginal voter" or "status politics" theorists. Finding that Wallace backers were more likely to feel politically powerless than partisans of other 1968 candidates,

but not more anomic or powerless with respect to their personal lives, and that Wallace emphasized and manipulated such anti-Washington impulses in his speeches, Carlson contends that feelings of powerlessness, especially about matters of racial policy, provide the key to Wallace's appeal.

But Carlson's conclusion is not the only possible interpretation of her data. Wallace supporters were disproportionately poorly educated (pp. 53, 168, 238), white rural (p. 89) people of Southern birth or residence (pp. 46, 88, 159, 259), who were much more racist than other whites, even those who favored Goldwater in 1964 (pp. 48, 94–98, 120, 164, 245, 261). In neither socioeconomic traits nor attitudes did they change much over the twelve-year period (pp. 253–68). Although not marginal in their parochial neighborhoods, they were increasingly irrelevant in the country's urbanized, cosmopolitan, and technocratic society. The survey questions she analyzes can no more rule out such a less simple-minded marginality hypothesis than they can enable one to determine whether the Wallace people's sense of powerlessness was, as she asserts (p. 171), or was not merely a reaction against the national government's move toward racially egalitarian policies. Although she puts a fancier label on it, one cannot, on the basis of Carlson's evidence, dismiss the contention that Wallace supporters were principally just backwoods racists.

An opportunistic posturing politician who moved aside from the schoolhouse door and later, when he needed black votes, tricked Rosa Parks into having her picture taken with him (p. 187), Wallace built no enduring organization, purveyed no consistent ideology, and, according to Carlson, left no political legacy (p. 287). With Ronald Reagan and the "New Right" in power, the last point seems less certain than it must have when this book went to press. Future historians may find what Carlson terms Wallace's "masquerades" (pp. 63, 67) to be prologues to Reagan's more substantial drama, Wallace's a "shadow movement" to Reagan's real conservatism, Wallace's supporters less satisfied with symbolic gestures against blacks, national social programs, and "weakness" in foreign policy than she presents them.

J. MORGAN KOUSSER
California Institute of Technology

JAMES T. PATTERSON. *America's Struggle against Poverty, 1900–1980*. Cambridge: Harvard University Press. 1981. Pp. ix, 268. \$17.50.

During the last twenty years or so the historical literature on social welfare has been dominated by those who have asserted that the nation's public and private welfare institutions and organizations have

been designed and used by the upper classes not to help the needy but to direct and control their behavior. In fact, so pervasive has this notion become, that David Rothman, one of the authors of a recent and very articulate statement on the "limits of benevolence," has suggested that there now even exists a widespread and acute suspicion of the very idea of doing good (Willard Gaylin *et al.*, *Doing Good: The Limits of Benevolence* [1978]). *America's Struggle against Poverty, 1900–1980* is the first scholarly work in some time to counter, implicitly and explicitly, this "social control" thesis. For this and for a variety of other reasons, it is a very important book.

James T. Patterson set out to do a number of things and succeeded in doing all of them exceptionally well. He presents a broad demographic and social history of the poor, an account of the changing causes of poverty during the twentieth century, an exegesis of prevailing attitudes toward poverty and welfare, both by reformers and the general public, and an analysis of public policy in the area, especially since the 1930s, when the federal government became increasingly involved in social welfare.

In so doing, he has put together the best analysis this reviewer has seen of the complexities of defining and dealing with the poverty problem, not just in quantitative but qualitative terms. Unlike many others who have written on the subject, he does not homogenize the poor, nor does he fail to deal with rural poverty; indeed, he explains why others have tended, over time, not to pay much attention to it. In addition, he periodically places the American experience within the context of international developments, another very useful facet of the study.

Several themes recur throughout the work, most of which are neither surprising nor novel. These include persistence of the beliefs among the nonpoor that the destitute are undeserving, that large numbers of them exist in an intergenerational "culture of poverty," that social insurance (or contributory schemes) are preferable to welfare handouts, which are wasteful and demoralizing, and that public policy should seek to prevent destitution rather than merely provide maintenance to the needy. All of these, of course, are based on the assumption that poverty can be eliminated, that it can be made to "wither away," to use the author's often-repeated phrase.

Patterson's chief contribution, however, is his ability to demonstrate that, however widespread and firmly held, the above ideas and attitudes have not always been the primary determinants in shaping public policy. Nor was public policy shaped primarily by a corporate elite's desire to bolster the work ethic and preserve the capitalist system, or by social unrest and turbulence among the nation's poor, as members of the social control school would have us believe.

Rather, according to Patterson, America's welfare state, which, by the way, he argues is considerably more advanced and responsive to the needs of the poor than many critics are willing to concede, is the product of a variety of factors, including good intentions. Most important, however, are economic forces, both the collapse of the economy, which initiated the welfare state in the 1930s, and unprecedented economic growth, which prompted, and permitted, many advances in the late 1960s and early 1970s; demographic changes, especially the aging of the population, mass movements from rural to urban areas, and a dramatic rise in the number of broken families; and finally, a number of political and institutional forces, especially the nationalization of politics, the growing number of pressure groups (particularly among the aged), and, significantly, the bureaucratic expansion of established federal programs and agencies. Contrary to the views held by many, in other words, including members of the National Welfare Rights Organization, the "welfare establishment" was not the enemy; HEW was a liberal bureaucracy that sought to aggrandize itself, and welfare recipients often benefited from its success, according to Patterson.

This work, then, extremely well written and based on an impressive array of sociological and economic as well as historical materials, is a welcome antidote to the prevailing literature on the subject. It must be read by all students and scholars of social welfare history and twentieth-century public policy. It should be mentioned, too, that the book demonstrates that sound social history still can be written without resorting to quantification and the other methods of the so-called new social historians.

WALTER I. TRATTNER
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CANADA

L. F. S. UPTON. *Micmacs and Colonists: Indian-White Relations in the Maritimes, 1713–1867*. Vancouver, Canada: University of British Columbia Press. 1979. Pp. xvi, 243. \$22.00.

In this work the late L. F. S. Upton opened an important topic for further study. In the standard English-language histories of the Maritime provinces of Canada, the Micmacs are either not discussed or are merely mentioned in passing. No one treating the history of the region will be able to ignore them in the future.

Upton begins by describing the way of life of the Micmacs from the period of contact to 1713. Their relations with the original French settlers were

good, since the latter occupied lands that were of no value to the Indians and were too few in number to pose a threat. French missionaries early converted the Micmacs to the Roman faith, to which they clung tenaciously during the succeeding centuries.

After the conquest of New France, with their source of military aid and logistical support removed, the Micmacs were no longer able to resist the flood of rapacious settlers from New England, especially after the coming of the American Revolution brought a horde of loyalists, all of whom believed that the lands of the Micmacs belonged to the British crown. No thought was given to the Indians as their lands were parceled out or occupied by squatters. This is the main theme of the book: the cruel, unthinking injustice inflicted on these people, who were shunted aside to starve in squalor. By the mid-nineteenth century the colonial authorities were able to contemplate with equanimity the final resolution of the Micmac problem: they would shortly be extinct.

One compelling chapter is devoted to the Micmacs and the churches. Attempts were made by various Protestant sects to wean the Micmacs away from Rome, but to no avail. As the author states, "A Catholic Indian did not have to become a white man; settlement was highly desirable but not essential to the soul's salvation. The English, however, thought differently for no protestant could distinguish between assimilation and salvation: the two were as one" (p. 160).

Yet the Micmacs survived and, as the author points out in a brief epilogue, they have now begun to fight back. Their numbers have increased from some three thousand in 1841 to about nine thousand in 1970. They have also adopted the novel idea that they had rights to their ancestral lands that could not be disposed of against their will. The courts of law are finally being forced to take cognizance of this claim, which was never accepted by the British in the seventeenth century.

Despite the emotion-charged nature of his subject, Upton wrote with complete detachment, letting the evidence speak for itself, and with his habitually graceful literary style. It has to be said, however, that the one map provided is pathetically inadequate; the lands of the Micmacs are not even indicated. The statement (p. xiii) that only the Micmacs fought for their lands is erroneous; so too did the Abenakis, all the other nations allied with the French, and those who subsequently followed Pontiac.

W. J. ECCLES
University of Toronto

C. P. STACEY. *Canada and the Age of Conflict: A History of Canadian External Policies. Volume 2, 1921-1948: The Mackenzie King Era.* Buffalo, N.Y.: University of

Toronto Press. 1981. Pp. x, 491. Cloth \$30.00, paper \$12.50.

Herewith C. P. Stacey concludes his two-volume survey of Canadian external relations since Confederation. Volume 2 begins in 1921 and ends in 1948—a quarter-century for most of which W. L. Mackenzie King dominated federal politics as prime minister. Since the Canadian prime minister simultaneously held, until 1946, the secretaryship for external affairs, that person had a unique opportunity to influence the tenor of Canadian external relations. King made the most of his opportunity, and this volume is aptly subtitled *The Mackenzie King Era*.

Arguing that foreign policy begins at home, Stacey sets forth the social, economic, and political conditions that governed Canadian reaction in areas of trade, defense, and autonomy to external problems generated by the world's age of conflict. A second contextual element is the decline of Britain and its replacement by the United States as the main center of Canadian external ties. This shift Stacey neither laments nor praises but considers logical, given the conditions of the times. Stacey has studied an era in which a "British dominion with uncertain and shaky international status came to be recognized, and to recognize itself, as an undoubted, independent nation—in a modest way, a power in its own right" (p. 425).

But history is character as well as circumstance. It was King who presided over the maturing of Canadian nationhood. Stacey, on this point, develops a thesis he had already expounded on in general form in his *Mackenzie King and the North Atlantic Triangle* (1976). Mackenzie King was a Victorian Canadian with a deep appreciation of British institutions. King argued that Canadian interests might not require a direct involvement in such diplomatic brush fires as Chanak (1922), but at a moment of fundamental crisis, such as the outbreak of the Hitler war in 1939, there would be no question as to the response Canada would give. Sensitive to the divergent elements in the nation, King worked overtime to maintain national unity. That he brought the country into World War II as united as it was, is rated by Stacey as King's greatest political achievement. Stacey deals with the character element superbly. His history is alive with the influences generated by King, Franklin D. Roosevelt, Winston Churchill, Arthur Meighen, R. B. Bennett, O. D. Skelton, and Loring Christie. The value of this factor is further heightened by occasional references to his personal circumstances and thoughts.

Thorough synthesis in attractive prose, based on sound research, is a mark of Stacey's qualities as a historian, and this volume continues the tradition. This study is based on Stacey's already deep dip into

the primary Canadian sources for earlier monographs such as *Arms, Men and Government* (1970). The rule regarding availability of secret government documents provided a second major reason for stopping this history in 1948. Stacey also demonstrates a thorough reading of the published, non-Canadian primary sources and of the secondary sources. For this generation this work should remain definitive.

W. H. HEICK
Wilfrid Laurier University

LATIN AMERICA

FRIEDRICH KATZ. *The Secret War in Mexico: Europe, the United States, and the Mexican Revolution*. Parts translated by LOREN GOLDNER. Chicago: University of Chicago Press. 1981. Pp. xii, 659. \$30.00.

This is an outstandingly good book. It is one of the most important yet published in the field of modern Latin American history. It is the most important on the Mexican Revolution since Frank Tannenbaum's *Peace by Revolution* (1933), the thesis of which—that the revolution was peasant, Indian, and nationalist—guided most scholarship on the subject well into the 1970s.

One of Tannenbaum's premises was that international factors figured in Mexican history simply as oppressive, that "the Mexican people" alone determined the revolution's course, from which the country emerged for the first time effectively sovereign. Friedrich Katz does not explicitly address Tannenbaum's argument. But he does note his own aim, "the integration of social and diplomatic history" (p. ix); in achieving it, he suggests a fundamentally different interpretation of the revolution.

The basis of Katz's book is the research that he did largely in European archives for his *Deutschland, Diaz und die mexikanische Revolution* (1964). But *The Secret War* is a substantially new study. The author has reconsidered the material he found earlier; searched several other major archives in Europe, the United States, and Mexico; mastered the many monographs that have appeared on modern Mexican history in the last twenty years; and produced a finely focused analysis of a broader and more complicated problem—"the whole fabric of international policies, the interplay between business interests and their governments, and their role in the political and social turmoil" in Mexico from 1910 to 1920 (p. ix).

Katz establishes beyond doubt the primary significance for the revolution of the Americo-European conflict in Mexico. The title expresses his essential insight. "The secret war" was not a wrangle between

spies, but "the new strategy of exploiting social conflicts and anticolonial struggles" that American businessmen and politicians had used in Cuba in 1898 and that European interests and powers adopted during World War I, for example, in the maneuvers involving T. E. Lawrence, V. I. Lenin, Thomas Masaryk, and M. N. Roy. Its development in Mexico during the revolution is a case "not only of how local rifts can be exploited for global ends, but of how global rifts can be exploited for local ends" (p. x).

The book has five parts. The first two emphasize the intensity of Anglo-American rivalry in Mexico from 1910 to 1914 and demonstrate its critical impact on both the Madero and Huerta governments. Part 3 comprises a deeply persuasive explanation of the split between the three main revolutionary factions and the eventual triumph of Carranza, a brilliant revision of the reasons for the Columbus raid, a judicious assessment of the punitive expedition's consequences in Mexican politics, and a fascinating account of German efforts to induce a U.S.-Mexican war, culminating in the Zimmermann telegram. Parts 4 and 5 are especially interesting, because they treat a period of revolutionary history scarcely examined before, 1917–20. Here Katz clarifies President Carranza's conservatism in domestic affairs, caution in foreign policy, and canny defense of at least the forms of Mexican sovereignty at a time when American interests and power threatened even them.

The most that remains of Tannenbaum's thesis is that the revolution was nationalist. But the greatest strength of Katz's book is precisely its proof of the international stakes in Mexican nationalism. This should redefine the major questions in modern Mexican social and political history for the next generation.

JOHN WOMACK
Harvard University

LINDA B. HALL. *Álvaro Obregón: Power and Revolution in Mexico, 1911–1920*. College Station: Texas A&M University Press. 1981. Pp. xiv, 290. \$22.50.

Meticulously researched, carefully structured, and nicely written, Linda B. Hall's study of the pre-presidential career of Álvaro Obregón is both a biographical endeavor and an inquiry into the nature of power and leadership within the Mexican revolutionary setting. With greater success than any previous author, Hall demonstrates how Obregón transformed an unquestioned reputation for military prowess into one of political respectability.

Relying heavily on manuscript sources, published documentation, a broad sampling of important sec-

ondary works, and the rich collection of oral history interviews gathered and organized by Eugenia Meyer at the Programa de Historia Oral, Hall traces Obregón's career from his early military campaigns against the Orozquistas in Sonora to the consolidation of his power under the Plan de Agua Prieta. To what does she attribute Obregón's meteoric rise from the obscurity of the garbanzo farmer in Huatabampo, Sonora, to the presidency in Mexico City? The answer is not found on any single page, but it is there: charisma, intelligence, military acumen, ability to compromise, the cultivation of excellent relationships with middle-class groups and northern elites, and fortuitous circumstance.

The decade covered in this study is not an easy period to understand, much less render comprehensible. Repeated fragmentation of revolutionary coalitions prompted civil war and near anarchy. The cast of important characters is large and their relationships to one another complex. But Hall copes well with both the personal antagonisms and the ideological cleavages. What emerges is the best study to date of Obregón's role in the traumatic ten years prior to the revolutionary reconstruction that began in 1920.

Illustrative of an approach that is both interpretative and synthetic is Hall's treatment of the Convention of Aguascalientes. Students interested in the convention should still turn first to Robert E. Quirk's masterful monograph, *The Mexican Revolution, 1914–1915*, without question the best treatment of the topic. But Hall's analysis, though indebted to Quirk, has a different set of purposes. She portrays the convention as a learning experience for Obregón in which he became acquainted with the wide spectrum of revolutionary aspirations. It was also a forum in which he forged new political ties and, most importantly, an episode that, in spite of outward appearances, precipitated distrust and hostility with the first chief, Venustiano Carranza. This examination helps explain the always-strained Obregón-Carranza alliance and ultimately Obregón's role in the revolt of Agua Prieta.

Although not confronting the issue directly, Hall surely disagrees with Douglas Richmond's newly emerging interpretation of the period 1915–20. Although viewing the Constitution of 1917 as a harbinger of process yet to unfold, she cannot accept the idea that the Carranza presidency initiated the process of revolutionary stabilization, much less consolidation. For Hall the continuities of the age of violence were not seriously challenged until Obregón assumed the high office in 1920. Having displayed her historical talents on getting Obregón into power, Hall will, one hopes, write a second volume devoted to that badly misunderstood administration.

MICHAEL C. MEYER
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STEVEN E. SANDERSON. *Agrarian Populism and the Mexican State: The Struggle for Land in Sonora*. Berkeley and Los Angeles: University of California Press. 1981. Pp. xx, 290. \$22.75.

The concept of populism has most frequently been applied in Mexican history to the Cárdenas era of the Revolution of 1910. Steven E. Sanderson has audaciously attempted to develop a theory of populism applicable to the entire revolutionary experience. According to Sanderson, the bourgeoisie was too weak as a class to promote capitalist development by itself and therefore developed an ideology of state populism to wed capitalist development with social and economic reform. As a consequence, the Mexican nineteenth-century Liberal state gradually transformed itself into an interventionist arbiter of both economic reform and social justice. The author uses a case study of Sonora to exemplify how the state manipulated agrarian populism as a tool of control to mobilize as well as demobilize the peasantry to further the ends of the bourgeoisie.

This monograph essentially presents a cursory survey of the relationship between civil society and the state from independence until 1976. The first part traces the roots of "state populism" or "capitalist populism," which are used interchangeably, to the inherent weakness of the progressive bourgeoisie after independence and its inability to gain control of the state. As a consequence bourgeois ideology envisaged the state assuming many of its "progressive" functions in promoting capitalist development. Acting as the "temporary surrogate of the bourgeoisie" the state began to intervene in the economy to uphold the sanctity of private property by abolishing corporate and communal landholdings during the Liberal and Porfirian eras. The revolutionary era from 1910 to 1940 was essentially a bourgeois revolution, Sanderson argues, but the bourgeoisie was still too weak to assert its control over the state and was obliged to forge a "populist pact" with the lower classes. The state acted as the "neutral arbiter of a multi-class coalition" promoting capitalist expansion and social reform. Since these goals were contradictory, the state acted as a class conciliator in the political sphere to minimize class conflict in the civil society. Peasants and the urban workers were mobilized from above to control their organizations, and minimal social reforms were instituted to reduce class conflict. The post-1940 period saw the emergence of a civil society dominated by an agro-industrial bourgeoisie that was stronger than the state. As a consequence, populism no longer functioned as a state policy, for the bourgeoisie refused to support both capitalist development and socio-economic reforms. More specifically, Luis Echeverría's agrarian reform policies in Sonora represented the last attempt by the state to revive populism in the face of rising bourgeois opposition.

This monograph makes an admirable attempt to analyze the relationship between the development in Mexico of civil society and the state since independence, which no other study has yet done. Yet the author falls short of his goal. Most importantly, his theoretical analysis is weak and at times contradictory because of his inability to define clearly the concept of populism and the exact nature of the state. By concentrating on the prerevolutionary and revolutionary policies of Mexican leaders, he neglects in his historical analysis to study the actual development of social class structure that is so vital to the understanding of the concepts of civil society and populism. For Sanderson the state is not the creation of the bourgeoisie but the reverse. It is a neutral arbiter trying to create, one surmises, a pluralist system. But how can a state follow an ideology of capitalist populism guaranteeing the future hegemony of the bourgeoisie if it is neutral? Redundancy rather than clarifying these two basic issues makes them more obscure. Most assuredly the original part of Sanderson's work is his case study of Echeverría's agrarian populism in Sonora. His conclusion regarding the ability of the bourgeoisie to abort the state's program of land reform will obviously provoke further discussion and research.

HEATHER FOWLER SALAMINI
Bradley University

MANUEL A. MACHADO, JR. *The North Mexican Cattle Industry, 1910–1975: Ideology, Conflict, and Change*. College Station: Texas A&M University Press. 1981. Pp. xiv, 152. \$14.50.

In this book, Manuel A. Machado, Jr., describes the complex factors—regional, national, and international—that have determined the characteristics of the cattle industry in northern Mexico during the twentieth century. He catalogues the depredations of the Revolution of 1910 that largely destroyed the huge holdings of livestock that had been controlled by a few powerful cattlemen, a number of whom were U.S. nationals. He describes the political instability, economic uncertainty, widespread pillaging, and contradictory policies of revolutionary governments that brought the U.S. government and cattle interests in that country into the picture in a series of diplomatic encounters during this period. In the 1920s, cattlemen and the government sought sporadically but largely unsuccessfully to rebuild the industry to its prerevolutionary scale. Further difficult years followed for the large national and foreign cattle interests because of the redistributive agrarianism of the Cárdenas administration. Although the government became noticeably more supportive of the industry and the landowners in

the decade that followed, by the late 1940s serious outbreaks of hoof-and-mouth disease decimated cattle holdings and brought renewed U.S. intervention in efforts to control its effects. The period from 1960 to 1975 is characterized as one of greater government support for the industry and a renewed emphasis on its export orientation.

Machado, a great admirer of the "norteño spirit" of independence and rugged individualism, is no friend of the Mexican Revolution or of its national and agrarian aspirations. In his discussion, the revolution resulted in the intrusion of a centralized government apparatus into a region that had a long history of localism and resistance to Mexico City. Nationalism during the revolution and ensuing years is repeatedly characterized as xenophobia, attempts to expropriate foreign cattle interests are dismissed as arbitrary and capricious, and the export orientation of the industry is unchallenged in terms of its implications for national economic and political development. Moreover, for this author, *ejidatarios* and small landowners simply lack technical and other skills necessary for a thriving livestock sector. Thus, for example, he claims that "[t]echnologically ignorant peasants, spurred on by opportunistic *caciques* (bosses), clamored for a just distribution of productive lands and betrayed their ignorance about practical agricultural economics" (p. 50).

As a result of perspectives such as these, readers will miss a serious analysis of the socioeconomic ramifications of land concentration and foreign ownership and intervention that have resulted from the industry's development. The book provides useful detail, especially on the diplomatic history of an industry linked economically, biologically, and technologically with the United States. Overall, however, its contributions are limited by the lack of an analytical framework that would have encouraged the author to assess critically the broader implications of the cattle industry's development.

MERILEE GRINDLE
Brown University

THOMAS P. ANDERSON. *The War of the Dispossessed: Honduras and El Salvador, 1969*. Lincoln: University of Nebraska Press. 1981. Pp. xii, 203. \$15.95.

In 1969 Thomas P. Anderson was residing in El Salvador, completing his work *Matanza*, when he found himself in the middle of what was to be called the Soccer War. He capitalized on the experience and began interviews and research that led to the volume under consideration here. Most Latin Americanists will be the beneficiaries of this circumstance; probably the work is too detailed for the "general reader," but it will provide the rest of us

with all the lecture material we will ever need on this tragic war.

Anderson accurately assesses the causes of the war, although to blame the failure of the 1824 federation is to stretch the use of *post hoc, ergo propter hoc*. The fundamental causes were boundary disputes, uneven results of the common market, and the heavy migration of Salvadorans into Honduras. These were mostly peasants looking for land plus a few small businessmen seeking better profits than they found at home. These latter, in my experience, were bitterly hated by their hosts.

Anderson believes that three hundred thousand Salvadorans moved into Honduras in this century. The practice was generally accepted until Honduran problems grew too large, and the safety valve was locked down. A new land reform program gave the government an out. By expropriating lands occupied by Salvadorans, Honduras could benefit its own landless without touching the oligarchy. El Salvador could not stand the return of so many of its people in the best of times.

Then sports intervened. Until 1969 the two nations had never faced each other in World Cup trials, and the rivalry was unbelievable. This was no mere World Series. The first two matches—home and home—brought out the worst in the Central Americans as locals pulled unmentionable stunts on visiting teams.

Unfortunately thousands of Salvadorans were then streaming across the border on their way home, crying theft, rape, and murder. Anderson discounts much of the reporting, but he was a rare neutral in El Salvador just as I was in Honduras (and how regrettable that we did not know of one another's presence). But the natives believed what they wanted.

Anderson details the four days of warfare, the peace moves, the long-range consequence almost foreshadowing today's news. He calls one chapter "El Salvador: Coming Apart" and the next "Honduras: Progress and Problems." They should be read in the White House or at least by Tom Enders.

My objections are confined to style. Some choppi-ness is created by the many suggestions that we must go back and look at so and so. There is cuteness: the Dutch skate along, worse is frequently to come: the economy is described as "yes, we have no bananas," and on page 173 is the implication that Anderson invented the term "Colossus of the North." Yet on page 11 is as succinct and accurate a description of El Salvador as one could ask. A tougher editor would have made this an even more valuable study, but is there any significance in the absence of Costa Rica from the maps of Central America?

THOMAS L. KARNES
Arizona State University

CHRISTIAN SCHNAKENBOURG. *Histoire de l'industrie sucrière en Guadeloupe aux XIX^e et XX^e siècles*. Volume 1, *La crise du système esclavagiste, 1835–1847*. Paris: L'Harmattan. 1980. Pp. 254. 72 fr.

Despite the importance of the sugar question for the history of the West Indies, relatively few historical studies exist on the Caribbean sugar industry. The dearth of historical literature on this issue is particularly great concerning the French West Indies. This lacuna has now been partially filled, however, by the work of Christian Schnakenbourg, an economic historian who previously did a doctoral dissertation on Guadeloupe's eighteenth-century sugar establishment. Indeed, Schnakenbourg's current book, covering the years 1835–47, is only the first of a proposed four-volume series that will eventually trace the development of Guadeloupe's sugar manufacturing from the 1830s to the end of the Second World War, thus providing a long-range analysis of a staple whose production has always constituted Guadeloupe's major economic activity.

Schnakenbourg's study examines two closely intertwined structures, the premechanized Guadeloupe sugar industry and French colonial slavery, during a phase of crisis and decline. The author begins with a section in which he briefly discusses the essential underpinnings of the early nineteenth-century sugar-based Guadeloupe economy—the plantation system, its slave labor, sugar production methods, trade relations with metropolitan France—before going on in the central part of his book to scrutinize the problems that sugar manufacturing experienced under the French July Monarchy. At a time when colonial cane sugar was facing increased competition from beet sugar produced in mainland France, the growing attacks on slavery by French abolitionists joined with slave discontent on Guadeloupe itself to undermine confidence in the future of the island. The result was "structural blockage" of Guadeloupe's sugar economy, for financiers refused cheap credit, planters' costs augmented, the glut of sugar meant lower prices, and reduced income brought increased debts at ever higher interest rates, obstructing capital investment and modernization. As Schnakenbourg demonstrates persuasively in the final section of his book, the key factor in the decline of the French colonial sugar industry prior to 1848 was the existence of slavery, which impeded all attempts to modernize production techniques because it failed to provide qualified labor at the same time that it destroyed credit. Only after the abolition of slavery by the Second French Republic in 1848 could large cane-processing plants take hold in Guadeloupe and eventually revitalize the French West Indian sugar industry.

The author is perhaps too modest when he suggests in his introduction that many of his findings are tentative and will soon require revision. To be sure, parts of his work appear weaker and less definitive than others. For example, Schnakenbourg's evidence concerning slavery and slave abolitionism tends to be rather thin at times. Then, too, he offers little factual support for his contention that the slaves themselves played a decisive role in undermining the French West Indian slave system. Nevertheless, the sections of Schnakenbourg's book that treat the conjunctural development of Guadeloupe's economy are quite impressive, and, although some aspects of his study might require modification by future scholars, his general conclusions will probably remain unchallenged for the most part. As a whole, then, Schnakenbourg's contribution is a valuable one, providing historians with a sound economic study of Guadeloupe's sugar industry and making a thought-provoking correlation between sugar and slavery.

LAWRENCE C. JENNINGS
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ROBERT BRENT TOPLIN. *Freedom and Prejudice: The Legacy of Slavery in the United States and Brazil*. (Contributions in Afro-American and African Studies, number 56.) Westport, Conn.: Greenwood Press. 1981. Pp. xxvi, 134. \$23.95.

This volume is explicitly and consciously framed in terms of the debate over the origins of differing patterns of race relations in the United States and Brazil that Frank Tannenbaum and then Carl Degler made part of the American historical tradition. Robert Brent Toplin has taken a group of essays, most of which were published previously in scholarly journals, and has attempted to create from them an integrated volume on slavery and race relations from the era of slavery to the twentieth century. Although the goal of the volume is admirable and some of the essays are quite informative, the book does not achieve its main objective because it is limited in the scope of its investigation.

Toplin organizes the volume into three sections. Part 1 examines the transformations in racial opinions in the South and Brazil. These essays are, in fact, the heart of the volume, because Toplin is interested above all in the attitudes of the slave-owning class. He reviews the major underpinnings and growth of scientific racism in the United States, but his chapter on Brazil does not really confront the lack of this mode of thought in that nation. Toplin suggests that the North American racial attitude toward mulattoes hardened in the nineteenth century. Although he does not mention the

disappearance of the mulatto category from the decennial census after 1850, that occurrence surely is an indication that he is right. Why this happened remains an unanswered question in this volume. Still, isolating the "mulatto question" is a real contribution.

In part 2 Toplin is on familiar ground. He provides a brief sketch of the abolition process in Brazil drawn from his earlier work and then tries to see the impact of the Brazilian situation on the perception of Southern slaveholders. Although this speculation is interesting, the parallel and perhaps more important influence of U.S. abolition on the perception of Brazilian slaveholders does not receive similar treatment.

In the final section on race and class in the twentieth century, Toplin lays bare some of the double-talk that long characterized evaluation of the Brazilian "racial paradise." The two chapters that constitute this section were originally written in 1971-72, and current events in Brazil, especially the development of self-conscious black movements, well aware of the gains of the civil-rights movements in the United States, make these chapters somewhat dated. Still, Toplin does identify the major themes in the comparison between Brazilian and U.S. race relations and the depth of feeling that this debate generates.

Although there is much of value in *Freedom and Prejudice*, its origin as a group of disparate articles published over the course of a decade is not overcome. There is repetition of points and sources and a certain unevenness that the author's introduction does not surmount. Perhaps Toplin's major achievement is to demonstrate that the legacy of slavery in the United States and Brazil is indeed a shared one and that there is advantage not only for historians but also for the citizens of these two nations to realize the unities and differences in their past and present.

STUART B. SCHWARTZ
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THOMAS FLORY. *Judge and Jury in Imperial Brazil, 1808-1871: Social Control and Political Stability in the New State*. (Latin American Monographs, number 53.) Austin: University of Texas Press. 1981. Pp. xiii, 268. \$19.95.

Thomas Flory has produced a readable book on the difficult subject of the nineteenth-century Brazilian judicial system, a topic that few foreign scholars have addressed. The work revolves around two major themes: first, that the interplay between liberalism and conservatism early in the century

shaped the political institutions of the monarchy and sparked ideological battles, including one over the judicial system, and second, that by 1841 the defeat of liberalism resulted in the nationalization of the judicial system by letting the justice of the peace occupy a peripheral position. It is the liberals' inability to defend local autonomy, embodied by the Constitution of 1824 in the justice of the peace structure, that makes up the core of the book.

In the 1824 constitution, the justice of the peace was designed to be the unpaid elected "neighborhood" judge at the parish level and to hold jurisdiction over petty civil and criminal cases. Flory informs us that these justices came from the ranks of local panjandruns. In the urban areas, merchants and artisans held the office, whereas in the rural upcountry it was monopolized by the landlords (pp. 70–71, 77–82). These elites were thus able to extend their private social and economic power into the public arena by holding this office. As such, the justice of the peace oversaw the maintenance of the status quo of a society where slaves and other members of the lower classes were looked upon as a constant source of disorder and outlawry. Stated simply, the local elites needed this kind of authority to stay in power.

Although Flory perceptively explains the process of liberal and conservative ideological debates over the judicial system, which both parties saw as an instrument for constraining the emperor's power, he is less successful in his treatment of the period 1841–71, that is, the victory of the conservative judicial reform of December 1841 and its consequences on imperial politics at large. The justice of the peace was literally written out of existence in the Reform on the Criminal Trial Code of 1841 (which Flory translates as the Procedural Code). In its stead, the troika of district judge, district police chief, and prosecutor became the vehicle for the delivery of imperial justice (pp. 172–73). In his discussion of the 1841 law, Flory does not mention its *Regulamento* (no. 120 of January 31, 1842) that stipulated how to enforce the first law. Nor does he inform the reader that the thirteen-article 1841 law failed to mention the office of justice of the peace. In fact, nowhere in the book does Flory demonstrate that he is aware that the Brazilian judicial system required two laws (a *lei* or *decreto* plus a *regulamento*) to cover a specific situation.

Flory's other observations can be easily misconstrued. For instance, the justice of the peace did not hold jurisdiction over cases involving the illicit slave trade even after 1850 (pp. 100–01); the enforcement of the anti-slave-trade law was assigned to the navy. The author is also undecided about the effects of the *Ato Adicional*. In one place, he interprets them as "centrifugal" (p. 134), in another "not at all centrifugal" (p. 158). Furthermore, Flory should not have confused the landlord-*juiz de paz*'s concern for order (p. 80) with his partisan preference. Not all the "established" oligarchs were conservatives, especially in Rio, Bahia, and Pernambuco (pp. 182–83); liberal power bases were as strong and prevalent as conservative ones in the Paraíba Valley of Rio province and in the Recôncavo of Bahia. Flory also mentions nothing of the Liberal party crisis of the 1860s and its consequences on the judicial system and thinking.

If social control by the judicial system was indeed one of the themes of the book—as the subtitle suggests—should not Flory have made some reference to the *juiz de orfãos*, the probate judge who officiated estate settlements and was as responsible for social and economic stability as the justice of the peace was in an agrarian society that had no rules governing entail or primogeniture? Flory also does not seem to be aware that the empire had other means of manipulating judicial officials than through the power of transfer (pp. 184–85) and the authority to grant or withdraw salaries (p. 177)—alternatives that included, among others, the setting of fixed terms of office.

These are some of the more salient aspects of the judicial system that the book fails to address, especially for the period after 1841. Had Flory confined his analysis of the vagaries of the imperial justice system to an intellectual history up to 1841, the book could have been superior. As it is, it suffers from uneven research, in particular in the areas of case studies. Flory does put one myth to rest, however. Contrary to popular belief, there were indeed fundamental differences between the nineteenth-century Brazilian liberals and conservatives, albeit philosophical ones.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

ROBERT I. ROTBERG and THEODORE K. RABB, editors. *Climate and History: Studies in Interdisciplinary History*. Princeton: Princeton University Press. 1981. Pp. 280. \$5.95.

REID A. BRYSON and CHRISTINE PADOCH, On the Climates of History. JAN DE VRIES, Measuring the Impact of Climate on History: The Search for Appropriate Methodologies. HELMUT E. LANDSBERG, Past Climates from Unexploited Written Sources. ANDREW B. APPLEBY, Epidemics and Famine in the Little Ice Age. CHRISTIAN PFISTER, The Little Ice Age: Thermal and Wetness Indices for Central Europe. JEROME NAMIAS, Severe Drought and Recent History. DAVID HERLIHY, Climate and Documentary Sources: A Comment. JOHN D. POST, The Impact of Climate on Political, Social, and Economic Change: A Comment. JOHN A. EDDY, Climate and the Role of the Sun. THOMPSON WEBB III, The Reconstruction of Climatic Sequences from Botanical Data. HAROLD C. FRITTS, G. ROBERT LOFGREN, and GEOFFREY A. GORDON, Past Climate Reconstructed from Tree Rings. ALEXANDER T. WILSON, Isotope Evidence from Past Climatic and Environmental Change. DONALD G. BAKER, Botanical and Chemical Evidence of Climatic Change: A Comment. DAVID HACKETT FISCHER, Climate and History: Priorities for Research. THEODORE K. RABB, The Historian and the Climatologist. EMMANUEL LE ROY LADURIE and MICHELINE BAULANT, Grape Harvests from the Fifteenth through the Nineteenth Centuries. BARBARA BELL, Analysis of Viticultural Data by Cumulative Deviations.

RICHARD RUBINSON, editor. *Dynamics of World Development*. (Political Economy of the World-System Annuals, number 4.) Beverly Hills, Calif.: Sage. 1981. Pp. 264. Cloth \$22.50, paper \$9.95.

MICHEL-ROLPH TROUILLOT, Peripheral Vibrations: The Case of Saint-Domingue's Coffee Revolution. KOSTIS PAPADANTONAKIS, The State as Instrument of Induction to the Periphery: The Case of Greece. RICHARD TARDANICO,

Perspectives on Revolutionary Mexico: The Regimes of Obregón and Calles. WALTER GOLDFRANK, Essence and Variation: Approaches to the Study of Contemporary Brazil. NESAR AHMAD, Agrarian Causes of the Iranian Crisis. JAMES E. LUNDAY, Political Regionalism and Struggles for State Hegemony. FRANCISCO O. RAMIREZ and GEORGE M. THOMAS, Structural Antecedents and Consequences of Statism. DALE L. JOHNSON, Intermediate Classes in a Bipolarizing Structure. ALBERT BERGESSEN, Long Economic Cycles and the Size of Industrial Enterprise. ROBERTA M. SPALTER-ROTH and EILEEN ZEITZ, Production and Reproduction of Everyday Life. SALLY K. WARD, Dependency, National Economy, and Inequality. TERENCE K. HOPKINS and IMMANUEL WALLERSTEIN, Structural Transformations of the World-Economy.

MICHAEL PALUMBO and WILLIAM O. SHANAHAN, editors. *Nationalism: Essays in Honor of Louis L. Snyder*. Foreword by ARTHUR SCHLESINGER, JR. (Contributions in Political Science, number 65; Global Perspectives in History and Politics, number 1.) Westport, Conn.: Greenwood Press. 1981. Pp. x, 219. \$27.50.

BOYD C. SHAFER, Webs of Common Interest: Nationalism, Internationalism, and Peace. THOMAS SPIRA, Nationalism: Recent Research and New Opportunities. GEORGE SCHWAB, State and Nation: Toward a Further Clarification. KONSTANTIN SYMMONS-SYMONOLEWICZ, Some Observations about the Comparative Approach to Nationalism. PETER SUGAR, From Ethnicity to Nationalism and Back Again. CHRISTOPHER J. CROWLEY, Luces and Hispanidad: Nationalism and Modernization in Eighteenth-Century Spain. WILLIAM O. SHANAHAN, A Neglected Source of German Nationalism: The Confederation of the Rhine, 1806-1813. BÉLA K. KIRÁLY, The Springboard of Young Ferenc Deák's Liberal Nationalism: The Emancipation of Hungary's Serfs. HANS L. TREFOUSSE, Carl Schurz and Ethnicity in America. ABRAHAM ASCHER, Pavel Axelrod's Dilemma of Jewish National Consciousness. PETER PASTOR, The Nationality Policy of the Hungarian Revolutionary Government, 1918-1919. DONALD J. HARVEY, Challenges to Nationalism in Twentieth-Century France. MICHAEL PALUMBO, The Italian Fascist Suppression of the Ethiopian National Resistance. ROMAN ROME, Louis L. Snyder: A Bio-Bibliographical Essay.

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TO THE EDITOR:

I have no quarrel with the central argument of the informative article by Steven Hause and Anne R. Kenney, "The Limits of Suffragist Behavior: Legalism and Militancy in France, 1876–1922" (*AHR*, 86 [1981]: 781–806). My comment is on that part of their analysis based on a comparison with the suffrage campaign in Britain. My principal published source is Jill Liddington and Jill Norris's important work, *One Hand Tied behind Us: The Rise of the Women's Suffrage Movement* (London, 1978), which Hause and Kenney unfortunately dismissed in a footnote. My own research in progress confirms and extends Liddington and Norris's findings.

The thrust of Hause and Kenney's argument is that, in Britain, the socialist, working-class component led to the militancy of the Women's Social and Political Union, and the implication is that this militancy led to success. I suggest that the working-class component was indeed of importance, contributing methods, personnel, and direct involvement—but that where this contribution was encouraged and effective was not in the WSPU, but in the so-called nonmilitant wing, the National Union of Women's Suffrage Societies.

While conceding that "the WSPU was unmistakably bourgeois during the period of its greatest notoriety (1906–14)," Hause and Kenney placed great emphasis on the origins of the organization, which was "closely associated with working-class politics in Manchester," claiming that "many of the early leaders and a large proportion of the founding members were workers" and that "the Pankhursts learned their militant tactics from their involvement in socialist politics" (p. 390).

Who were "the early leaders" of the WSPU? Early, middle, and late, the identifiable leaders in any true sense were the Pankhursts (and finally, only Christabel and Emmeline Pankhurst). Well before 1906, "the Pankhurst absolutism [was] . . . already obvious—and feared" (Teresa Billington, as quoted by Andrew Rosen, *Rise Up, Women!* [London, 1974], 63). And, despite early socialist connections, the Pankhursts' style was their own.

Liddington and Norris have shown that there was, throughout the textile area, and centered in Manchester, a strong, growing, working-class women's movement for suffrage, a movement that began ten years before the Pankhursts started to attract attention, and largely ignored by them until 1903. The "radical suffragists" had gained their political education, and their education in methods, in trade unions, the Women's Cooperative Guilds, and, to a lesser extent, the Labour party. They had addressed meetings, campaigned in Clarion vans, organized impressive petitions and deputations, lobbied members of Parliament, voters, and local politicians, and worked within the trade unions for successive goals. The Pankhursts' brief dalliance with this movement (1903–05) was characterized by disruptions, not only of Liberal party meetings, but (unintentionally) of the organizations and campaigns carefully built by the textile workers themselves. They made some recruits, but far from learning from the workers they gained little understanding of the all-important relationship between suffrage and feminist issues in the workplace. When they went to London, the movement continued without them.

Hause and Kenney are rightly interested not merely in the more spectacular aspects of the Pankhursts' campaign, but in the extensive use in the British movement of legitimate pressure methods eschewed by the bourgeois French women as lacking in respectability. With few exceptions, these methods were not used, let alone brought to any sophistication, by the Pankhursts and the WSPU. It was the NUWSS, "liberal" and "middle-class" (p. 789) as it had been (and still in part remained), that forged the essential alliance with working-class suffragists—and refrained from trying to make them

over in their own image. From 1906 on the NUWSS took on a new lease of life—and the attention-getting, consciousness-raising activities of the Pankhursts deserve some (but not all) of the credit for this—becoming a very lively, growing organization indeed, opening new branches, building membership, raising funds, establishing political connections, revising its constitution, and reviving and extending its ties with the radical suffragists. Working-class members were numerous and respected, and experienced women were hired as political organizers. The methods they had used were adopted and expanded. Still largely unwritten is the story of the extraordinarily all-encompassing political pressure machine that grew up on this basis of grass-roots campaigning, dovetailed with the central political work that was the contribution of some of the middle-class members of the NUWSS; the winning over of the Labour party to a firm commitment is only one of its triumphs.

I plan to develop elsewhere the evidence for my conviction that the work of the NUWSS laid the ground for success. By 1910, escalating militancy was counterproductive. Unfortunately, it was deemed newsworthy, and historians are only just beginning to understand what lay behind the headlines. Hause and Kenney's ready acceptance of the myth is disappointing, especially as the truth would serve their argument almost as well. For the women's suffrage movement in Britain did succeed in large part because of the cooperation between working-class and middle-class suffragists, the willingness of the latter to learn from the former, and the adoption by a national organization of tactics already in use by workers' groups. These were not, however, the illegal and disruptive tactics favored by the Pankhurst-led WSPU, but the planned strategies of conversion and pressure used by the women textile workers and brought to fruition by the NUWSS.

JO VELLACOTT
University of Toronto

TO THE EDITOR:

I was pleased to note that my book, *Screening Out the Past* (1980), inspired Robert Sklar to review it twice, first in *Cineaste* (June, 1981), and more recently in the *AHR* (86 [1981]: 945). And I was flattered to read that he considers my reinterpretation of early film and cultural history "promising in conception," despite its implicit criticism of his own published views. I was less happy, of course, when in the latter part of "Review II" his professional generosity dissolved into a factually inaccurate assault on my scholarship. A commitment to historical accuracy compels me to set the record straight.

One source of contention is our different views of

reform and the movie industry during the Progressive era. Like many others, Sklar has argued that reformers had little influence on the movies. But my research in the papers of the first major censorship agency, the National Board of Review, and in the industry's trade journals suggests an important and previously unrecognized relationship, which flourished between 1908 and 1914, the crucial period when the movies moved out of immigrant areas into middle-class neighborhoods. Producers who wanted to enter new markets sought the board's seal of approval and even paid its expenses, for in cities like New York reformers had pressured city governments to license theaters based on the criteria of cleanliness and positive recreation. Citing evidence from the board's papers, I wrote that it "approved approximately 600 films a month. During October, 1914, its members reviewed 571 films, eliminated 75 scenes, 10 reels and three entire movies" (p. 55). Since nearly the entire output of the industry came before the board, the influence of reformers was real. In stating that I provide no evidence for this view, Sklar ignores this documentation, as well as over 50 cited sources, including statements from critics, social workers, theater owners, censors and audience studies (pp. 43–67).

The intertwining of reformers' goals and the movies reached one culmination in the career of D. W. Griffith, a connection which Sklar disputes, presumably because only "three [films] are mentioned briefly." Had he read my book with reasonable care, Sklar would have noted that on page 66 I too state that Griffith made hundreds of films. In my search for their pervasive concerns, I drew not only on Griffith's published writings and papers, but cited twenty-five of the more than one hundred films and plots examined at the Museum of Modern Art and the Library of Congress. I then selected seven of his early, and eight of his post-1914 works (correlated with ten others in the footnotes) to illustrate the "general" themes of all his films (p. 78). Sklar can legitimately question the examples I chose, but he cannot state that I studied only three.

Once the National Board's influence faded after 1914, we face the important problem of when a modern style consistently appeared in the movies. The argument I present is that Jewish entrepreneurs, who built the major corporations by selling that style, entered the industry early, but they did not dominate until the end of the decade (pp. 169, 176–177). Charging me with altering the "basic chronology" of the industry to "buttress my thesis," Sklar continues to insist that Jews were in "control" by at least 1913. But a careful analysis of the trade journals in that year shows that no single set of firms as yet controlled production, supply, and distribution as did the future "Big Eight" firms that would come to dominate Hollywood. In 1913, the indus-

try's sixty or more companies produced over two thousand films. Of these, less than 20 percent can be clearly identified as having originated from Jewish producers. That fact is not too surprising since Adolph Zukor of the future Paramount Pictures had just made his first feature in July, 1912, and Louis B. Mayer of the future MGM had not entered a production company until 1915, and did not make his own features until 1918.

Finally we come to the serious question of accuracy. In the process of writing my book, I and the editors at Oxford Press strove to overcome the problems of informal documentation that plagues the field of the film history. Most books are broad, synthetic histories based on secondary sources. Few footnotes are provided, and it is customary not to cite the archival depository of the analyzed movies. This is doubly unfortunate in an industry where it is not unusual to change plots, endings or titles for different audiences, and where personalities changed their names to fit altered tastes. Even Sklar's well-known book follows this informal format. For example, in his discussion of the Jewish moguls' rise, there are nineteen pages of the most controversial material devoid of footnotes (pp. 36–44, 140–151), and the generalized nature of the bibliographical essay makes it almost impossible to identify specific sources. So when Sklar asserts that I have altered plots, titles, and dates, it is possible we are using different archival material, or one of us is citing the film's release date, the other the year it was finished. Interested readers can at least locate my sources from the footnotes.

As to the accusation that I have misspelled names, I do not claim ultimate perfection. But is Sklar referring to such "errors" as the spelling of William and Cecil B. De Mille? In his book both brothers are written *De Mille*, but I found through De Mille family sources that William spelled his name *de Mille* and thereby distinguished himself from his brother.

In Sklar's next book, or perhaps in his next review of mine, he would be well advised to cite his sources clearly and fully. Then, as he notes in the various reviews of *Screening Out the Past*, he might not need to complain that "film specialists have sometimes been castigated, and rightly, for failing to meet the standards of professional historians."

LARY MAY
University of Minnesota

PROFESSOR SKLAR REPLIES:

Professor May's book contains numerous errors both of fact and of interpretation. The errors of fact give clear warning that there are larger, more important problems in interpretation and the handling of evidence.

When I concluded in my review that "it is almost scandalous that a scholarly press should permit so many errors to go uncorrected into print," I was referring to the failures of editing by his publisher. Some scholars are slipshod, but vigilant copy editing and proofreading can save them from considerable embarrassment. Unfortunately, Professor May appears to have benefited from neither of these.

Here, for example, is a footnote citation (p. 293 n. 8). The article cited is by Patricke Johns-Heine and Hans H. Gerth, "Values in Mass Periodical Fiction, 1921–1940," in Bernard Rosenberg and David Manning White, editors, *Mass Culture: The Popular Arts in America* (Glencoe, Ill., 1957). It appears in *Screening Out the Past* as Patriche Johns Heine and Hans G. Girth, "Values in Mass Periodical Fiction, 1921–1941," in Bernard Rosenberg and David White Manning, *The Popular Arts* (New York, 1957). There are at least seven separate errors in that citation.

This is but one of many such errors in *Screening Out the Past*. Harry Aitken's name is spelled Aitkin through the book, on at least six separate occasions. Griffith's film, *The Song of the Shirt*, released in 1908, is given a 1910 release date by May on two separate occasions (pp. 79 and 275 n. 35). Charles Chaplin's *Modern Times* is dated 1935 in the text (p. 239) and 1936 (p. 296 n. 4), the latter correctly. Nathanael West's novel, *The Day of the Locust*, is dated 1930 (p. 278) and 1939 (p. 167 and p. 296 n. 3), the latter two correctly. King Vidor's film, *The Crowd* (1927), is dated 1925 (pp. 216 and 295 n. 30), and the description of the film (pp. 216–17) is egregiously in error, as anyone familiar with the film would immediately note.

These are a sample of the factual errors in *Screening Out the Past*, some errors in scholarship, some possibly typographical errors. Not one of them has to do with "the problems of informal documentation that plagues [*sic*] the field of film history."

Yet as I wrote in my review, the more serious problems of *Screening Out the Past* are problems of evidence and interpretation. Take the subject of D. W. Griffith. The point in my review was that Professor May does not cite sufficient evidence, particularly from the Griffith films made at Biograph from 1908 to 1913, to demonstrate his "linkage" thesis. Including the footnotes, I count nine such films briefly mentioned throughout the work (pp. 78–79 and 275), one, as noted, misdated, and another, *The Battle at Elderbush Gulch*, incorrectly cited as *The Battle of Eldrich Gulch* (p. 275 n. 34). This in no way represents the variety and complexity of the more than four hundred films that are available to scholars and that at this point in the development of film history need to be fully taken into account.

Professor May misquotes my review when he adds

the year 1913 to my statement about Jewish immigrant businessmen gaining control in the industry. My point was that his book alters the chronology of developments in the motion picture industry to support his thesis. To refute my point, he equates the motion picture industry only with producers, ignoring the important role of distribution companies and exhibitors in the pre-Hollywood years. He cites "trade journals" as the source of his evidence, apparently unaware that the trade journals were partisans in the conflicts within the motion picture industry and thus need to be carefully evaluated as sources. Professor May's letter is of a piece with his book.

ROBERT SKLAR
New York University

TO THE EDITOR:

The suspicion and accusations that attended the reign of the Yung-cheng Emperor (r. 1723–35) seem to have affected some scholars who study that period today. Professor Pei Huang's review (*AHR*, 85 [1980]: 962) of my *Passage to Power: K'ang-hsi and His Heir Apparent, 1661–1722* (1979) asserts that "some of Wu's major points are identical to those in my *Autocracy at Work* (1975), which devotes more than 30 pages to the succession problem. But nowhere does Wu's book cite it or list it." Huang plainly implies that I plagiarized him.

The fact is that my first book on the K'ang-hsi/Yung-cheng era, *Communication and Imperial Control in China, 1693–1735*, published in 1970, five years prior to *Autocracy at Work: A Study of the Yung-cheng Period, 1723–1735*, was planned as part of a series. *Passage to Power*, second part of the series, was developed out of certain chapters of my first book and my views and interpretations in both books are essentially the same. The original manuscript of this work because of length had to be further split into two volumes. Consequently I did not include Huang's book in the bibliography of the present volume on K'ang-hsi, but rather chose to deal with it in the next volume on Yung-cheng, along with other secondary sources, as being more pertinent to the Yung-cheng succession controversy (*Passage to Power*, p. 226). I regret this seeming oversight.

At the same time I must also note that Huang's attack on my work has been going on for almost a decade. (See his review of my first book and my rejoinder in *Harvard Journal of Asiatic Studies* [1971] and [1972].) Professor Jonathan Spence, in the *AHR* (81 [1976]: 933–34) felt obliged to note that some remarks in Huang's *Autocracy at Work* "seem to draw significantly on the conclusion of Wu's book" while at the same time being "consistently dismissive" of my work. And in Huang's review of my present

book, for example, his accusation that I cited "actually nonexistent sources" is ridiculous. The "nonexisting" Chinese book in question, *Li Wen-cheng kung/ [Kuang-ti] nien-p'u* (A Chronological Biography of Li Kuang-ti), is listed in both of my books (which Huang reviewed) and is readily available in America. Huang is perfectly entitled to express his views on how to cite Chinese works; but it is totally unprofessional to charge that my book relies on fabricated sources. Huang's other tortuous and inconsistent allegations against my present work can easily be disproved to the satisfaction of any fair-minded reader willing to take the time. In the space available here I can only deny them for the record. For the sake of the field (not to mention saving precious space in the *AHR*), I hereby urge Huang to devote his time and talents to further research and publication, rather than continuing his *ad hominem* attack on me.

SILAS H. L. WU
Boston College

PROFESSOR HUANG REPLIES:

Professor Silas Wu's letter attempts to deflect the crucial questions I raised in my review of his *Passage to Power*. Contrary to his assertion, only in chapter 6 does his first book, *Communication and Imperial Control in China*, touch on Yin-jeng, the heir apparent. Its remaining chapters center around the palace memorial system. Because of different focuses these two books do not share the same views or interpretations. As for Jonathan Spence's review of my book, I urge Wu to read my communication (*AHR*, 82 [1977]: 1379). What is really in question is *Passage to Power*.

The basic issue is Wu's failure to document the points he drew from my *Autocracy at Work*. Were these minor points, I would consider it a case of "fair use." Nevertheless, they underlay Wu's conclusion about the succession controversy, the subject of *Passage to Power*. For instance, his discussion of competition between the imperial sons, Yin-chih and Yin-chen (pp. 165–67), are identical in both content and wording to the analysis in my book (pp. 71–76). Like other Western sources, Wu's first book renders the eldest imperial son's name as "Yin-ti" (pp. 59 and 60). In his *Passage to Power*, Wu changes the name to "Yin-shih," the transliteration first adopted in my book (pp. 27, 313 n. 4). Wu's argument about the possible designation of the fourteenth imperial son as a new heir apparent (p. 175) also follows the same reasoning and phrasing in my book (p. 79). If given more space, I could provide more examples. I wonder how Wu, when confronted with mounting evidence, can dismiss such a serious issue merely as "seeming oversight."

Another important issue concerns various kinds

of nonentities. The book *Li Kuang-ti nien-p'u*, cited in *Passage to Power* (pp. 217 n. 10, 223 n. 45), does not exist in any library, not even in Wu's own bibliography. Frequently Wu cites nonexistent volume numbers, for example, the Korean source on page 219 note 1, and nonexistent page numbers, for example, the second reference on page 197 note 15 and the last reference on page 204 note 1. Citations on page 219 note 57 and page 221 note 15 have nothing to do with Wu's arguments. Worst of all, in discussing the final defeat of Yin-chih in the succession struggle, Wu uses the words and comments, such as "single-handed" and some other ideas (pp. 166–67), that are identical to those in my book (p. 74). But he assigns the analysis to the prince's "contemporary observer" and provides on page 222 note 27 a source, throughout which I cannot find the "observer" or the same idea.

In several cases, Wu tailored the contents of imperial decrees. For example, a decree in the K'ang-hsi *Veritable Records* (227:3b) mentioned that after long peace very few imperial officers knew naval warfare. This would affect, the same decree maintained, the future security of Taiwan. Wu writes: "... there were indications of a possible

revolt in Taiwan, which might swell into an invasion of the mainland" (p. 94). None of the coups (pp. 84, 152) or sexual adventures (pp. 91, 113–15) can be found in the sources he furnished. Altogether, I filled six typing sheets with such nonexistences and changes. The similar problem also occurs in his first book, which I just finished a second reading. Whatever their nature, I found, all these nonentities and changes fit his thesis perfectly. Of course, Wu knows well that generally the readers will not examine the documentation of his book. So far, he has "successfully" published two books, with a third one in the making.

After receiving a copy of Wu's letter to the editor, I reread the *Passage to Power* twice. I found that my review did not do Professor Wu any injustice. My criticism is not "dismissive remarks" or a personal attack. It is rather of an academic nature because Wu's topic has been part of my major interest since 1956. I strongly believe that a historian should have a sense of responsibility not only to his readers but also to his own standards. The profession requires accurate presentation and honest documentation.

PEI HUANG

Youngstown State University

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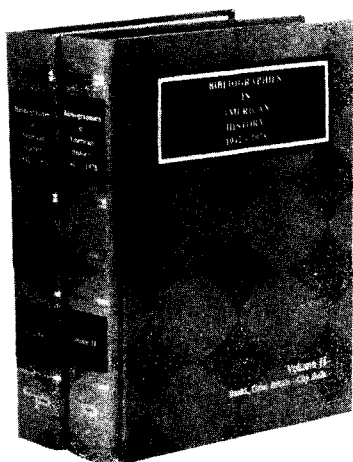
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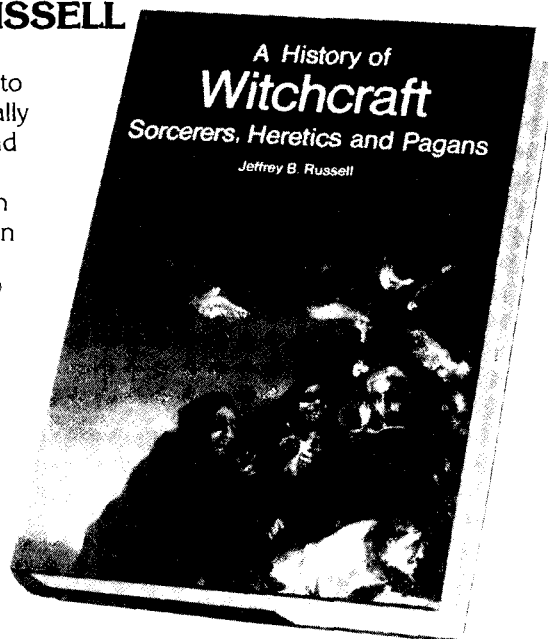
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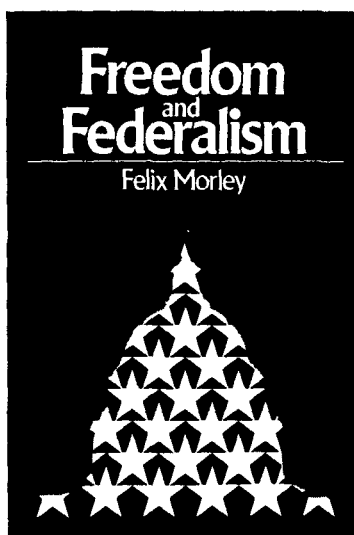
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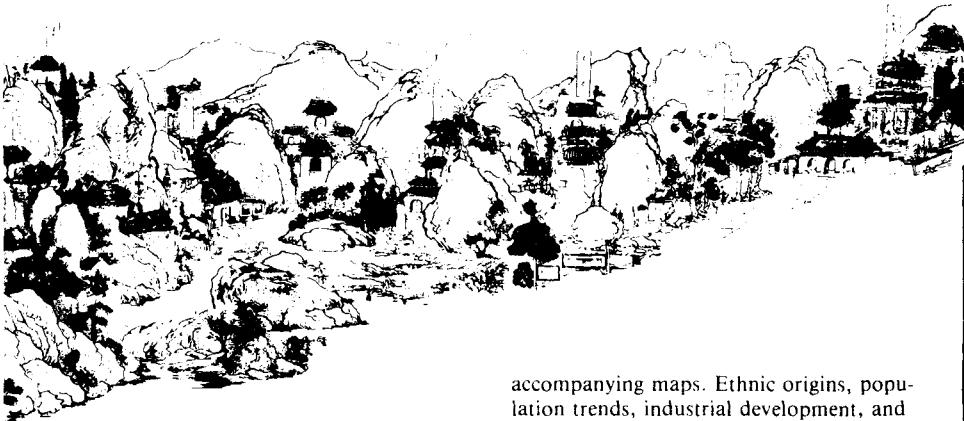
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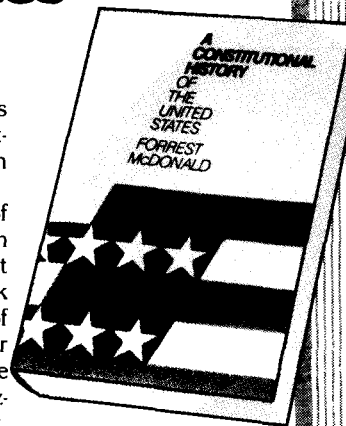
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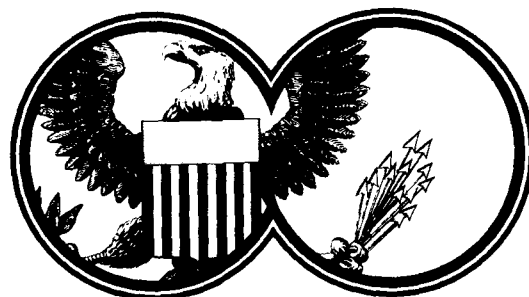
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